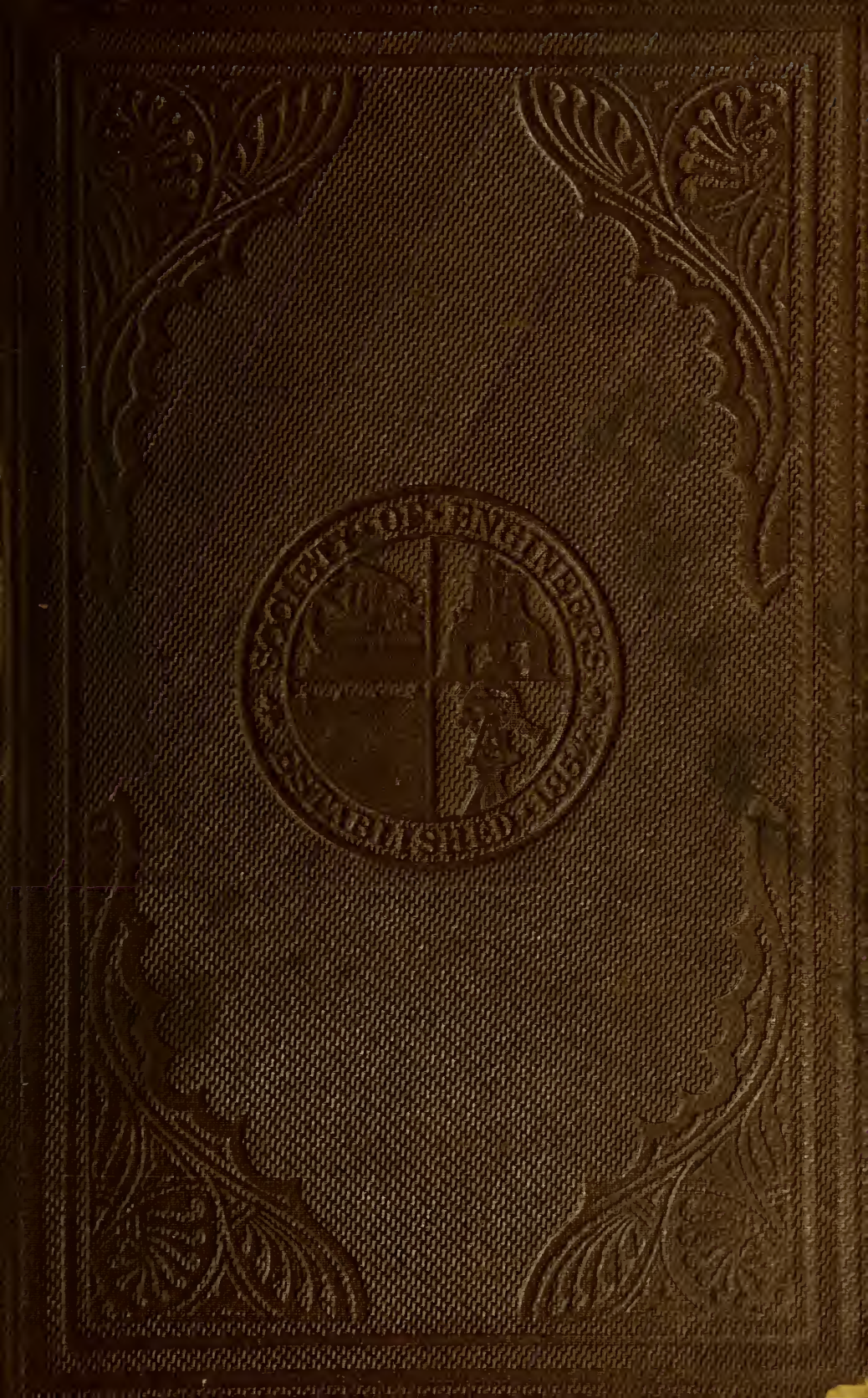
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SOCIETY OF ENGINEERS.

ESTABLISHED MAY, 1854.

TRANSACTIONS FOR 1886.

EDITED BY

CHARLES J. LIGHT, MEMB. SOC. ENG.,

SECRETARY.

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PREMIUMS FOR 1886.

At a meeting of the Society, held on February 7th, 1887, The Bessemer Premium of Books was awarded to:

PERCY TARBUTT, Assoc. M. Inst. C.E., for his Paper on "Liquid Fuel."

The President's Premium to:

GEORGE B. JERRAM, for his Paper on "River Pollution caused by Sewage Disposal."

And a Society's Premium to:

EDWARD SKELTON BELLASIS, Assoc. M. Inst. C.E., for his Paper on "The Roorkee Hydraulic Experiments."

McElroy
1917
6 Feb

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SOCIETY OF ENGINEERS.

ESTABLISHED MAY, 1854.

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PLACE OF MEETING:

THE TOWN HALL, WESTMINSTER.

1887.

TRANSACTIONS, &c.

February 1*st*, 1886.

INAUGURAL ADDRESS.

PERRY FAIRFAX NURSEY,

PRESIDENT.

GENTLEMEN,

Twenty-eight years of pleasant association with the Society of Engineers as a member, and, for a portion of that time, of active work as its secretary, have not diminished the sense of appreciation I have from the first entertained of the high honour which attaches to the office of President. On the contrary, this long connection has the rather served to intensify it and to deepen my present feeling of obligation to the Council for having nominated me, and to the Members in general, for having unanimously elected me to that office. I will not pretend that I have never indulged in the hope of one day occupying this chair. I have done so, and for the fulfilment of that hope, and for enabling me to realise what I conceive to be with every Member a legitimate and laudable ambition, I thank you most sincerely. If my long connection with our Society has made me acquainted with the privileges of the position I now have the honour to hold, it has no less familiarised me with its responsibilities. If my hopes and aspirations have been stimulated by the light of those privileges, they have also been held in gentle check by the shadow of those responsibilities. But in accepting the office of President, I accept freely and gladly all that pertains thereto, feeling assured that I shall receive both from my colleagues on the Council and from the general body of Members the same warm support and co-operation that has always been accorded to my predecessors in this chair. But for this support our Society could not have attained that prominent position which I unhesitatingly claim for it. With this support extended to me, I shall hope during my tenure of office to at least maintain the Society in its present position, although my best endeavours shall be used to advance its

status. It always has been and always will be my desire to promote its welfare, and if at the end of my year of office it should be found that I have fallen short of my own intentions and of your expectations, I can only leave the measure of my shortcomings to be filled up by your kind consideration. Once more I thank you for having placed me at the head of our Society, and I assure you I shall hope to merit the confidence you have thus reposed in me, by doing my best to uphold the dignity of this chair, and the honour of the Society which you have thus committed to my charge.

By virtue of an unwritten law, the President of this Society in preparing his inaugural address is unfettered by many of those considerations which affect other communications read before the Society and known as papers. So long as he observes the traditions of the Society by giving a brief reflex of the previous year's work, he is free to follow the bent of his own inclinations, and to disregard those hard and fast lines which have very properly to be observed by the authors of papers. Availing myself of this licence, which custom has sanctioned and which I trust I shall not be deemed to have abused, I purpose this evening in the first place to notice the papers read and the visits made by us last year, adding, in several instances, a few particulars respecting matters of cognate and subsequent interest. In continuation of this I shall bring under your notice a few more recent discoveries and developments in connection with some of our industrial processes, after which I shall touch upon various professional and scientific topics, which I hope may prove interesting. Here I shall, as it were, rove at will through the wide domains of science, culling a few stray flowers and scattering them, perhaps a little carelessly, amongst you. And I prefer to do this rather than to deal with the dry bones of some of the more exact scientific truths—dry only because they have been so frequently and so well picked by others. When I commenced to write this address, I did however pause to consider whether I ought not to attempt to invest some of these dry bones with a fresh interest—to endue them with a new life. But although I had the desire to succeed in starting or developing some new field of inquiry, I had not the temerity to make the attempt. I might, it is true, have twined a wreath of fancies around some occult subject, and baptizing it a new theory—as is sometimes done nowadays in the feverish race after notoriety—have presented it for your acceptance. But what have we to do with fancies? Our profession demands for its pabulum facts. We are essentially matter-of-fact men, and are never more happy than when, by a vigorous application of the besom of fact we succeed in sweeping away a delusive fancy.

First then let me turn to our Society. And here I must congratulate my predecessor upon the satisfactory condition in which he hands the affairs of the Society over to me. Notwithstanding an exceptional but unavoidable pressure upon our funds with which he had to deal upon taking office, by good management, supplemented by the efficient working of our secretary, he leaves our financial position substantially improved, as the annual statement of accounts passed by our auditor shows. The muster-roll of the Society continues to be well sustained; and notwithstanding the losses which we in common with all other institutions must inevitably experience from deaths, resignations, and other incidental causes, we are able to maintain a good front, by reason of fresh accessions to our ranks during the past year. Considering the long-continued depression with which we have been struggling and from which we have not yet emerged, our numerical strength, and in fact our position generally, is most satisfactory. Let me remind you that it is within the power of every Member to aid in still further improving that position by introducing Members, and if you will only take the matter up with the earnestness it deserves we shall advance rapidly in that as well as in all other respects. I do not forget that several of our Members have exerted themselves most creditably in this connection, and on behalf of the Society I thank them for so doing. I would suggest that Members in the provinces should look their friends up in this matter, for it is the experience of many of us that professional friends only want looking up a little to be induced to join us. And what is the experience of some would surely be the experience of all, if they would only give themselves a little praiseworthy trouble. I leave the matter in the hands of the Members, only asking them to support me by doing, or endeavouring to do, something for the Society in the direction I have indicated. All have not equal opportunities, but those who are favourably placed in this respect cannot be said to have the interests of the Society at heart if they neglect these opportunities when they arise, or even if they do not seek to create them.

The practical work done by the Society during the past year consists in the reading and discussion of a series of useful papers, exclusive of the interesting address of my worthy predecessor, Mr. Charles Gandon. Besides this, during the vacation we made three visits to works of engineering interest. The first paper of the year was by Mr. Arthur Rigg, one of our recent past-presidents, who favoured us with a highly interesting communication on "American Engineering Enterprise." This paper was compiled from observations made by Mr. Rigg during a visit to Canada and the United States in the previous

autumn. It consisted principally of general observations upon American machinery, with special reference, however, to the railways, locomotives, and turbines of the country. The mining interests in Montaine and Leadville were also touched upon. No one who was present at the reading of that paper will readily forget the striking photographs of the Rio Grande Mountain railway and other characteristic scenes with which Mr. Rigg illustrated his observations, nor the splendid collection of water-colour drawings which were kindly lent for the occasion by Mr. John Dixon, and by which the hall was completely surrounded.

The next paper was by Mr. J. Dixon Gibbs, on "The Distribution of Electrical Energy by Secondary Generators," in which the author described the Gaulard-Gibbs secondary generator system, which affords the means of transforming primary into secondary currents by utilising the phenomena of induction. The most substantial advance made in house-to-house electric lighting during the past year appears to have been made with this system. Mr. Gibbs in his paper told us of its successful application on the Metropolitan Railway in 1883, the primary circuit being composed of a single wire, 15 miles in length and only $\frac{1}{8}$ inch in diameter, and of the second application which took place in the following year in Turin. There the primary circuit was 50 miles in length and the wire only 3·7 millimetres in diameter and without a covering. I may supplement Mr. Gibbs' paper by observing that the most recent installation is that at the Grosvenor Gallery, where, for several months past, the secondary generator system has been in successful operation on a gradually extending scale, having been successfully applied to a number of establishments in Bond Street and the neighbourhood. This installation, when completed, will consist of the distribution of electrical energy to more than 5000 lamps of various systems, and will constitute the first practical and commercial example in England of *bonâ fide* house-to-house distribution of the electrical current from a central station. The plant and machinery for this purpose are placed under the Grosvenor Gallery, and consist of steam-engines of 600 horse-power indicated, and four dynamos of a type specially constructed for this system by Messrs. Siemens Brothers. The current is led to the various establishments by means of an overhead copper conductor, a secondary generator being placed in each house where the current is used. At Aschersleben, in Germany, an installation has recently been completed, and is working satisfactorily, comprising the distribution of 200 horse-power in light over a circuit of about 6 miles; some of the lamps being employed to light

a mine. In this case the first practical application of the system will be made to the distribution of mechanical power. The current supplied by the secondary generators is also to be used for electro-chemical purposes. The lighting of the town of Tours, in France, is about to be inaugurated, the installation of the first 200 horse-power plant and machinery having been completed. In Italy installations will be completed early in the present year for supplying light to the towns of Turin and Tivoli by the Gaulard-Gibbs system, and arrangements are in progress for transporting 2000 horse-power—derived from the waterfalls of Tivoli—from Tivoli to Rome, to be applied to lighting the principal part of the latter city. In the United States of America an eminent firm in Pittsburgh has undertaken to work the system. Facts like these indicate material progress in this system of electric lighting, a progress which I have no evidence to show has been made with any other system in this country.

Apropos of electricity, we have all heard of gas and water being filched from the mains by those whose ingenuity was greatly in excess of their morality, but I was hardly prepared to learn that the electric current had been similarly laid under contribution by this class of depredators. Yet such is the case, and the offence has been committed in that land of advanced thought and action—America. The theft was ingeniously planned and as ingeniously punished when discovered. The leakage to the earth of the underground leads of the Edison electric lighting system, from the central lighting station in New York, is about three ampères, the tension being 110 volts. It was found that numerous unprincipled persons had availed themselves of the opportunity to steal electricity, and use it for operating motors and for induction coils. The method of filching the electricity was by boring through the iron pipe surrounding the insulating compound, and then further into one of the copper leads; a set screw fixed into this orifice, formed one connection and the earth the other. Of course this connection was made beyond the electric meter. It was hardly worth while to maintain the continuous espionage necessary to detect and punish these pilferers, but the superintendent of the station coupled in extra dynamos and threw as great an increase of current over the system as the safety catches would permit, at various times for about one second. While this current was passing the lamps would give an unwonted glow, and every induction coil and motor surreptitiously attached to the system would receive an extra current designed to burn it. In this manner the system is occasionally cleared of all trespassers.

Reverting to our papers, I may observe that we were next favoured with a paper on "Cable Tramways" by Mr. W. Newby Colam, in which that gentleman described the various lines on this system in America, Australia, and New Zealand. Mr. Colam gave us an interesting account of the history of the invention and its application to the first cable tramway, which was constructed up Clay Street Hill, San Francisco, in 1873. It was shown that two lines had been working successfully in New Zealand for some years; that 27 miles of line had been commenced in Melbourne, and also another line in Sydney, Australia, and that lines were in course of construction in Philadelphia and other American cities. The New York authorities had also just sanctioned the laying of 72 miles of cable line in that crowded city. Mr. Colam then proceeded to give a detailed description of the construction of the Highgate cable tramway, of which he is the engineer. The Council awarded Mr. Colam a premium for his paper.

The subject of Portland cement was brought under our notice by Mr. Henry Faija, who observed that specifications for cement should be as simple as possible, and he advised the adoption of the ordinary quality of cement, obtaining the different strengths required by varying the proportion of sand or aggregate, in preference to specifying unusual qualities. He told us that the only points which needed to be tested were those of fineness, tensile strength, and soundness. He also observed that the best practice was to test the briquettes at three days after moulding, and again at seven days. At the former period they should not break under 175 lb. strain, or at the latter under 350 lb. We then had exhibited and explained to us a machine devised and used by Mr. Faija for gauging cement; also a testing machine, arranged to give the most advantageous rate of speed in applying the pressure, which was stated to be 100 lb. per fifteen seconds.

One of the most carefully prepared papers and the best illustrated one of the session was that by our secretary, Mr. Charles J. Light, on "Opening Bridges on the Furness Railway." In this paper we had brought before us two types of opening bridges, both possessing special features. The first was the Buccleuch Dock Bridge, a double-leaf swing bridge, carrying the railway and a carriage road and two footways across a water passage, 80 feet in width, between the Buccleuch and Cavendish Docks at Barrow-in-Furness. The other was the Ulverston Canal Bridge, a traversing bridge carrying the Bardsea Branch of the Furness Railway, at a sharp skew, across the canal. The points of special interest in the Buccleuch Dock Bridge are the spring girders, which constitute a new mode of construction for swing

bridges, and one possessing great advantages. The chief of these are the ease and rapidity with which the bridge is opened or closed (a point of the highest importance for railway purposes), while it rests when loaded, not upon rollers, but upon solid bearing-plates, at the coping of the water-way, the clear span being thus reduced to a minimum. In the Ulverston Canal Bridge the novelty is the arrangement by which the bridge traverses at right angles to the canal, reducing to a minimum the clear span of the girders, their weight, and the extent of traverse. Mr. Light showed the value of these reductions in the case of an opening bridge crossing at a skew (in the present instance) of 48°. The eminently practical character of this paper, and the care bestowed upon its preparation, and upon that of the diagrams, marked it out as especially deserving of a premium. The Members of Council, and the officers of the Society, however, being disqualified from receiving premiums, this distinctive mark of approbation had to be withheld from Mr. Light.

Another paper of considerable value to those members of the profession whose practice lies in the direction of docks and harbours was that with which we were favoured by Mr. J. B. Redman, on "Tidal Approaches and Deep-water Entrances." The author first pointed out the necessity for increased depths in our docks and shipping depôts, which had arisen from the rapid increase of late years in the size of our ships, as marked by the transition of freight and passenger vessels from the 1200-ton Indiaman to the 5000-ton steamer. Mr. Redman explained the peculiarity of British tides, and pointed out the great advantages they secured to this country. The character of various deep-water sites was analysed, and a comparison was drawn between the London and Liverpool estuaries. The navigable properties of the Thames reaches were described, and the difficulties of docking ships on a tideway referred to. A review was given of the rise, progress, and schemes for extension of our Metropolitan Docks. The new Tilbury Docks, now approaching completion, were considered, especially as regards the approaches from seaward. The modern improvements in dredging plant were referred to, as showing the direction in which engineers must look for artificial aid in opening out tidal approaches; and the author concluded his interesting paper with a brief notice of the various methods of deep-water foundations by cylinders, caissons, crib-work, concrete in blocks, *pierre perdue*, squared blocks, *en masse*, or by bags and skips, by monolithic hollow blocks, or the section of quay or wall entire, and pointed out how largely these various methods would be extended and developed in the future for deep-water entrances. Mr. Redman's

paper was profusely illustrated, and its excellence and practical character fully entitled it to the premium which the Council awarded to the author.

I would here add to Mr. Redman's paper a few particulars of a very important work on the Thames of a cognate character, which I recently had the opportunity of inspecting. This is the extension of the Galleons basin of the Royal Albert Dock, and the construction of a new deep-water entrance lock by the London and St. Katharine Dock Company, who are the proprietors of this and the Royal Victoria Dock adjoining. The necessity for this has arisen from the circumstance that the 30 feet of water over the sill of the original entrance lock at Galleons Reach is hardly sufficient at certain states of the tide for some vessels. The new entrance lock lies a little to the north of the other, and is 550 feet long by 80 feet wide, with a depth of 36 feet of water over the sill. There are three pairs of wrought-iron lock gates, with green-heart posts, working against Cornish granite hollow quoins and shutting sills. Cast steel has been used for the roller paths, gate rollers, and pivots. There are culverts beneath the lock to convey pipes, drains, electric and telephone wires. The gates, sluices, and capstans will be worked by hydraulic power. The basin has been extended from 12 to 15¼ acres, with two additional berths for vessels, so that the basin will accommodate six instead of four ships as at present. The lock and basin walls are of concrete with an outer facing of gault and blue vitrified bricks; the coping is also of specially made blue bricks. The works are protected on the river side by a cofferdam, parts of which form the solid arms of the pier-heads carried into the river; and on the dock side by the concrete wall of the north-east angle of the Galleons basin. This wall is 530 feet in length, and varies from 5 feet wide at top to 20 feet at bottom, with strong counterforts, and a toe about 5 feet wide. This wall is in course of removal by drilling and blasting; and the arrangements for its entire demolition without interfering with the shipping and general business of the docks form an interesting feature in the work. One of the most important of the new works from a commercial point of view, is probably the new river wharf, which is connected with the land at its upper end by the lower pier-head of the new entrance, and near its middle by a separate approach. It has a length of 1120 feet down the Thames, with a depth alongside of 27 feet at low water. The company are also embanking the foreshore of the river in the rear of and parallel to the wharf. The works have been entrusted by the Directors to Colonel Martindale, R.E., the general manager, and Mr. Robert Carr, the engineer to the company, with Mr. J. Thomas,

the assistant engineer, and are being carried out by them. It is anticipated that with the exception of the embankment of the foreshore the works will be completed and opened for use this spring, and the embankment in the course of the present year.

The last paper of the year was by Mr. Philip S. Justice, who introduced to our notice a new method of dephosphorising iron in the puddling furnace. This process is the invention of Mr. Joseph Beasley, and although it has only recently been brought under public notice, it has been in operation for more than two years at some works at Pensnett, Staffordshire, where more than 20,000 tons of bar iron of superior quality have been made entirely from Northampton and cinder pig. The process consists in using a special fettling, composed of calcined tap cinder and blue billy reduced to the condition of sand and mixed with dilute hydrochloric acid. This mixture stands for three or four days, during which time it is frequently turned and incorporated, certain proportions of common salt and ground lime being added shortly before use. These ingredients are thoroughly commingled, made into a stiff paste with water, and plastered over the ore or bulldog which is placed around the furnace bottom. Over this are thrown a few shovelfuls of a mixture composed of ground ore and ground hammer slag, saturated with hydrochloric acid prepared similarly to the first compound, and to which lime and salt are also added. With this fettling, and with a dose of oxide of iron administered towards the close of the puddling process, it appears that there is an increase of from 5 to 10 per cent. upon the weight of the pig charged into the furnace, and that the iron produced is free from red-shortness, shows a highly fibrous fracture equal to that of the best marked bars, and possesses a tensile strength of about 24 tons per square inch.

In connection with the subject of Mr. Justice's paper, I may here mention that during the past year a new process of steel-making has been started by Messrs. Bott and Hackney at Manchester. It is termed a direct process, and may be described as a compromise between the Bessemer and the crucible processes. An important advantage secured is that baked moulds are dispensed with, the castings being made entirely in green sand, so that the many severe internal strains caused by hard moulds at the time of cooling are avoided. Steel castings are thus produced which are practically free from blow-holes and shrinkage, notwithstanding that metal out of one ladle can be indiscriminately poured into elevator bucket moulds less than $\frac{1}{8}$ inch thick, or into moulds for heavy crank shafts. The Clapp-Griffiths is another steel process which has been

brought prominently forward during the past year. This may be considered as a pneumatic system similar to the Bessemer, but with the difference that the converter is fixed and non-tilting, and that the blast is introduced around the lower section of the converter instead of through its bottom. The object of the process, like that of several others of a similar nature which have been introduced of late years, is to enable small users of steel to become their own manufacturers without having to lay down an expensive plant. In America a new method of producing steel was reported to have been started by its inventor, Mr. W. A. O. Wuth, of Pittsburgh, the steel produced being, it is stated, of a high grade and substantially free from sulphur and phosphorus, and containing a definite percentage of carbon. Mr. Wuth's process consists in making the steel from wrought iron which is practically free from phosphorus, sulphur, and carbon, by melting it on an open hearth in contact with a form of carbon which will not oxidise at the heat necessary to smelt the iron, but which will unite with the iron at that heat, plumbago being the form of carbon used. By means of this process it was stated that Mr. Wuth had made several charges of steel of 12 tons each, containing as low as 0·01 per cent. of phosphorus, and that he would be able to produce any kind of fine steel that might be desired. I am not aware, however, that the process has proved a success either scientifically or commercially.

I cannot quit the present subject without referring to the loss metallurgical science experienced in the early part of last year by the death of Sidney Gilchrist Thomas, to whose genius and untiring labours the Thomas-Gilchrist dephosphorising process mainly owes its existence. Mr. Thomas, who was born in April 1850, died on February 1st, 1885. As a friend I have reason to know that whilst his genius found for his name a place in many lands, his gentleness found for himself a place in many hearts.

I have now taken you through the work of the year as regards the reading of papers, and in one or two instances have added a few particulars respecting cognate matters. Before leaving the subject, let me impress upon Members the importance of contributing to our Transactions. This importance is twofold, and refers to themselves as well as to the Society. To themselves, inasmuch as in the preparation of a paper some particulars bearing upon the subject have generally to be sought outside their own immediate knowledge. From repeated experience, I affirm that information is frequently acquired in this way which proves of practical value afterwards. The importance to the Society of producing each year a well-filled

volume of practical matter for the use of our country, colonial and foreign members, and of the profession at large, is so self-evident that I will not waste time by insisting upon it. I ask you then to support me in this respect during my year of office, and not only me but my successors in this chair. Do not suppose we require lengthy and elaborate treatises. A short carefully-written paper on some special detail of practice, or a description of some industrial process or useful invention, which affords opportunity for discussion and the interchange of opinion, is far more valuable than a long highly-finished communication, the only merit and the only fault of which consists in its unimpeachable perfectness.

I turn next to our vacation visits, of which we last year had three, as I have already observed. Our first visit was made on June 17th, to the new Deep-water Docks at Tilbury which are now nearly completed. The works comprise a river entrance 300 feet wide, from which two timber jetties extend forwards into the river. The tidal basin has a water area of 19¼ acres with a depth of 26 feet at low water, spring tides, and 46 feet from Trinity high water. From the tidal basin a lock 700 feet long and 80 feet wide opens into the main dock, which is 1600 feet long with a width of 600 feet, and has a water area of 23 acres with a depth of 38 feet from Trinity high water. From the main dock three branch docks project forwards, the whole forming a trident in plan. These branch docks have each a length of 1600 feet, the centre one a width of 300 feet, and the two side ones an average width of 250 feet each. The total area of the main dock and branches is 52½ acres.

Our second trip was to the locomotive and carriage works of the South-Eastern Railway Company at Ashford, which we visited on July 8th. There we were shown the details of locomotives, carriages, and waggons in course of preparation and manufacture, and the engines and rolling stock under construction and repair. A curiosity was shown to us in an old first and second class composite carriage which was used by the Duke of Wellington when Lord Warden of the Cinque Ports, and which presented a remarkable contrast to the splendid carriages of the present day. This relic of past railway practice measures 14 feet 9 inches long by 6 feet 8 inches wide, the first-class compartment, which is the centre, being 6 feet 8 inches in height, and the two second-class compartments 5 feet 5 inches high each. In the locomotive department we were shown a similar curiosity, in an old four-wheeled locomotive, the *Invicta*, bearing date 1830. This engine has a pair of sharply inclined cylinders 9 inches diameter by 18 inches stroke, and a single-flued boiler. Here was indeed a contrast to

Mr. Stirling's latest express passenger engine with 19-inch cylinders and a 26-inch stroke, which stood near.

Our third and last visit of the vacation was to the London Yard Engineering Works of Messrs. Westwood, Baillie and Co. The works in progress inspected there included several bridges for the Indian State Railways, the principal one being for a span of 800 feet. This bridge will be constructed over the Rohri Pass of the Indus at Sukkur, on the line of railway from Kurrachee and Attock. It is designed on the cantilever principle, similarly to the Forth bridge, than which of course it is much smaller. It is of one span only, but that span is 790 feet in the clear between the abutments, and 820 feet between the vertical pillars. There is a space of 200 feet between the ends of the two cantilevers which will be filled in by a girder, thus uniting the cantilevers and completing the span. The bridge will be constructed of steel, of which material the cantilevers will absorb 3200 tons; this is exclusive of the 200-feet girder in the centre.

It will thus be seen that the visits cover a wide and varied range of practice. That they are attractive and interesting is indicated by the numbers by which they are attended. That they are valuable is admitted by all, for they afford an insight into the detail construction of works which I do not hesitate to affirm would remain a sealed book to many of our Members but for these visits, which this Society takes credit for inaugurating, and which practice it intends to have the credit of maintaining, year by year, so long as those who are at present at its head are permitted by your favour to assist in directing its affairs.

I have alluded to the pleasure it affords engineers to sweep away fancies by a vigorous application of the besom of fact. Let me here endeavour to give a practical illustration to my meaning. A favourite theme with some of our most gifted writers and speculators is the probable exhaustion of our coal supply. Some twenty years since, Professor Stanley Jevons startled the world by his utterances in this connection, and his disciples have kept the ball rolling ever since by an occasional although feeble kick. Lately, however, one of our leading scientists—Dr. Lyon Playfair—has taken up the refrain, and this induces me to place before you a few facts which, if they do not negative this theory of the proximate exhaustion of our coal supply, at any rate to my mind place the consummation of the prediction at a more and more remote period almost year by year. Dr. Playfair referred to the subject in his inaugural address last year as President of the British Association. He states that, according to Professor Dewar, "in less than two hundred years the coal of this country will be wholly exhausted," and he goes on to

observe that our not very distant descendants will have to face the problem of a coalless England, thereby endorsing the opinion he had just quoted. Now with all due deference to the two learned professors, whose opinions are doubtless based on ample data, I must dissent from their conclusions. This I do on the ground that fresh discoveries of coal continue to be made both at home and abroad. Let me go back about nine years, namely to the close of 1876, at which time I visited the South Yorkshire coal-field and inspected several new workings. I there found that in round numbers over one hundred new collieries had been started within a comparatively short time of my visit. Of that number, there were in the Leeds district 31 new workings, in the Bradford, 6; in Halifax, 12; in Wakefield, 14; in Sheffield, 17; in Rotherham, 7; in Pontefract, 3; and in Huddersfield, 11. But it was the new workings in the Barnsley district to which my attention was specially directed, and there I found that seams of coal previously known only near the outcrop, were proved to retain their valuable qualities to depths far exceeding those previously reached in the district. This fact was established by the intersection, within a depth of 500 yards, of four seams of coal extending over an area of at least 15,000 acres, and which less than a year before my visit were absolutely unpierced in the district. Although this perhaps can hardly be considered a new discovery, yet it is comparable in value to the discovery of a new seam, seeing that the quality and thickness of coal beds are by no means constant. I have not noted the progress of coal development between 1876 and the present time, but I know that several discoveries have been made. Last year the Wombwell Main Colliery Company started to sink from the Barnsley bed to the Silkstone seam, which they expect to meet with at about 600 yards. This had been previously done on the opposite side of a well-known throw, but this is the first attempt to reach the throw on the Wombwell side, and the result is looked forward to with great interest, inasmuch as, if the quality and thickness are found all right, many thousand acres of coal unequalled for household purposes will be won.

Turning to the South Wales district, it is to be observed that an important discovery took place last September, of a very considerable deposit of valuable coal at Clydach Dale, in the Ynysybwl Valley, near Pontypridd. The seam is solid coal 6 feet in thickness, and was met with at a depth of 545 yards from the surface. The intersection of this seam in Clydach Dale is the most important discovery of coal made in South Wales for some time. It was supposed that, owing to a certain fault, coal in this valley was at an impracticable depth. Owing, however, to the spirited enterprise of Messrs. Davies, Scott and

Co., this supposition has been proved to be erroneous. From Mexico we learn that an extensive coal bed has been found near the line of the Mexican Central Railway, and about midway between El Paso del Norte and the city of Mexico. This discovery is of great importance to the country, for scarcity of fuel has been one of the most serious hindrances to Mexican progress. The mountains have been denuded of timber, and the railways have been buying English and American coal, and hauling the former from Vera Cruz and the latter from Indian Territory and New Mexico at an enormous cost. If coal becomes cheap and plentiful, the railway and mining interests of Mexico will doubtless increase greatly in value. The coal-fields of New South Wales have been further developed during the past year. A new colliery has been opened out on the Burwood Estate, and a new shaft has been sunk at New Lambton. Steps have been taken for opening out a new colliery, near Woolongong, whilst another mine has been opened out six miles north of Singleton, and near the Great Northern Railway. China also appears to be making an advance in respect of coal winning, for the Chinese government officials, wisely casting aside national prejudice, have determined to work the coal mines of the country in a more systematic manner than has hitherto been done, calling in the aid of European miners. They applied to the Société Cockerill, Belgium, for a contingent of experienced miners to superintend the extensive collieries which they propose to open up in certain of the rich deposits already prospected, and men were engaged. There are large deposits of coal in China, and, with the assistance now obtained, they will doubtless be rapidly developed. All these facts, and many others of a similar nature, serve, in my opinion, to postpone very considerably the verification of the predictions of the exhaustion of our sources of coal supply.

These utterances, however, are not wholly without value, inasmuch as they serve to direct attention to the development of other sources of artificial heat and light than coal. In this connection it may not be inappropriate if I here observe that the liquid fuel question has been advanced a stage during the past year. This question is by no means of so recent a date as some would think. It is more than half a century old; for so long ago as 1830 Mr. Pinkus—whose name is perhaps best known in connection with atmospheric propulsion—claimed to have used hydro-carbons in conjunction with streams of vapours for steam-raising purposes. From that date to the present engineers and inventors have not for any length of time ceased to labour in the same direction. I well remember Richardson's petroleum

furnace, as well as that of the cheery-hearted Bridges Adams, who worked so hard for years at the permanent way question. Both of these liquid fuel systems were for many years under the constant notice of the officials in Woolwich Dockyard, and in 1868 the Government gave Messrs. Wise, Field and Aydon permission to apply their system of using petroleum to a marine boiler on board a steam yacht. This system, in which the oil is burned by the aid of an induced current, had been previously applied to a Cornish boiler at some large works in London, where I was afforded opportunities of seeing it at work. The Admiralty also tried at Sheerness a somewhat similar plan, invented by Mr. S. E. Crow, but, as in the Woolwich trials, without anything practical resulting. In the same year—1868—which appears to have been marked by a sudden outbreak of inventive activity in connection with the subject of liquid fuel, Dorsett's petroleum furnace was fitted under the boiler of the s.s. *Retriever* of 90-horse power and 500 tons burthen, and I made one or two very successful runs in her. As far, however, as the adoption of any of these systems in England has gone, nothing appears to have resulted. The main reason for this I believe to be that directly a demand was created for the class of liquid fuel used, and which at that time was really a drug on the market, up went the prices to a prohibitive extent, and contracts forward could not be made except at a prospective heavy loss.

But the activity of inventive genius in the matter of liquid fuel in 1868 was not confined to England. In France corresponding attention was paid to the subject in the same year. The leading system tried there was that of M. Verstraët, which was applied to a locomotive on the Eastern of France Railway, and upon the occasion of the Emperor visiting the camp at Châlons the train was drawn by the engine thus fitted. His Majesty rode on the foot-plate of the engine in company with MM. Sauvage, Dieudonné, and Sainte-Claire-Deville. In the same year his Majesty also made a run in the *Puebla*, a steamer in which mineral oil was employed to raise steam for the engines. In America liquid fuel has been used both on locomotives and in steamers to a considerable extent. But notwithstanding the advantages offered in the way of a cheap and plenteous supply of liquid fuel, it would appear to have but a limited application in practice for steam-raising purposes at the present time. For plate-heating furnaces and in certain metallurgical operations I believe it is in use at certain works. In Russia, particularly in the oil regions, a very different condition of things exists, inasmuch as for some years past petroleum refuse has been advantageously used as fuel

in the locomotives of the Grazi and Tsaritsin Railway in South-East Russia. The system there adopted is that of Mr. Urquhart, the first trials having been made in 1874. Besides this, numbers of steamers are now running on the Caspian Sea with their boilers heated by liquid fuel.

The special advance made in England during the past year, to which I have alluded, has been effected by Mr. Percy F. Tarbutt, whose system has been adopted in the *Himalaya*, a trader of 800 tons burthen, and having engines of 100 horse-power. This system consists of an automatic starting arrangement, which provides for the supply of oil to the furnaces during the period of getting up steam without the aid of an auxiliary boiler, and an arrangement for highly superheating the steam which is used for forcing in the oil, and by its means raising the temperature of the fuel to the burning point as it enters the furnace. I recently inspected the working of this apparatus on board the *Himalaya*, which has since made a very successful run from London to Granton and back, and has now gone to Brazil, where she will ply along the coast for trading purposes. The Tarbutt system has also been fitted in a ship recently launched by Messrs. Wigham, Richardson and Co., and which is intended for the Black Sea trade. All this, however, does not bring us any nearer the solution of the liquid fuel problem at home, in the face of a limited supply and high prices. What does, however, promise to help us is the circumstance that in consequence of the comparative scarcity and dearness of petroleum in this country, a fleet of large tank steamers is being built by the Russian Black Sea Navigation Company to bring Russian oil to Europe in bulk. The two practical facts I have just stated, taken in conjunction, justify the expectation that before long liquid fuel will come into somewhat general use with us, thereby tending of course to the conservation of that invaluable source of energy—coal.

From the use of mineral oil for heating, to its application for lighting purposes, is an easy and a natural transition. In this latter connection I may here notice the progress made in lighting railway carriages by means of gas produced from the refuse of mineral oils. It is satisfactory to know that Pintsch's system of compressed oil gas is the one which is gradually superseding the solemn and effete old oil lamp on most of our leading lines. As a matter of fact, up to the present time no fewer than fifteen British railway companies have adopted the compressed oil-gas system. These lines have in the aggregate 400 trains fitted up with compressed oil-gas apparatus. The London and South-Western Company has decided to fit all its best rolling stock upon this system, and already some 500

or 600 carriages on this line may be seen running thus lighted. In Northern Germany the adoption of the system is so universal, that every railway carriage is fitted with oil-gas lamps and cylinders as naturally as it is with drawbars, springs, and wheels. When it is remembered that the wretched oil lamp costs as much as a halfpenny per hour and that an oil-gas light of three times the power costs considerably less, the adoption of the latter system would appear to be only a common-sense proceeding on the part of railway companies, both on the score of economy and in view of the boon it would confer upon that long-suffering section of the community—the travelling public.

Two important processes in connection with the textile industry have been perfected during the past year. These are the Frémy-Urbain process of preparing the fibre of the rheea plant for the spinner, and the Mather-Thompson system of bleaching. It has fallen to me professionally to follow and occasionally to report upon the gradual development of the Frémy-Urbain process during the past two years, and it was a pleasure to me to see Professor Frémy last year give it the finishing touch. The process as a whole commences with the decortication of the stems of rheea by steaming them in a box, according to a method invented by M. Favier. After having been steamed, the bark containing the fibre is stripped from the stems in ribbons. The fibre is then released from the ribbons produced by M. Favier by the purely chemical process of MM. Frémy and Urbain. This latter process consists mainly in treating the ribbons with certain alkalies, by which means the fibre is produced in a satisfactory manner for the spinner. I believe it will be found that we have in these combined processes the long-sought solution of an important industrial problem, namely, the disengagement of the fibre of the rheea plant from its resinous bed in a simple, safe, expeditious, and economical manner. The remarkable strength and lustrous appearance of this fibre, as well as the wide range of its application, have long pointed it out as an important material for textile purposes, and manufacturers have long coveted the prize. It is to be hoped that it will soon be placed within their reach, if indeed it is not already so to a limited extent, for the full benefit cannot of course be reaped until the cultivation of the plant has been extended. This, however, is being done in Johore, China, and other parts. The rheea plant is very prolific under equatorial influences, so that it is to be hoped that the scientific may soon be succeeded by the commercial solution of the rheea fibre question, by which a new staple will be given to the textile industry.

The second process to which I have referred is no less important than the first taken in connection with the industry it affects. This is the Mather-Thompson system of bleaching, which renders this operation a matter of a few hours, as against several days under the ordinary method. The process consists in first saturating the cloth with a dilute solution of caustic soda, and loading it in waggons which are run into steaming kiers, where the cloth is steamed for five hours. During the steaming a solution of caustic soda is injected at intervals into the kiers in the form of spray, and finally the cloth is washed in hot water, by means of circulating pumps, and removed from the kiers. It is then submitted to the bleaching process, in which the cloth is passed through a series of tanks between indiarubber tapes, encountering in each a certain chemical treatment. The process, which is continuous, is now in regular operation in Messrs. Ainsworth's bleaching works at Halliwell, near Bolton. The great value of the process consists in the fact that the three operations for what is known as "market" or "white" bleaching only occupy about twenty hours, and under pressure this time can be reduced to twelve hours. The results obtained, however, are equal to those produced under the old system by sixteen operations, occupying from two to four days. The process, which is of the first importance to the bleaching industry, is the joint production of Mr. W. Mather (of the firm of Mather and Platt, of Manchester), and Mr. Jacob Baynes Thompson. By Mr. Mather's arrangements Mr. Thompson's chemicking process has been developed up to its maximum efficiency.

There are many other interesting matters upon which I might here touch, but the history of scientific progress during the past year has been so recently and so well written in the various scientific journals, that I should probably only weary you were I to extend my record. I therefore pass on to other themes, and propose in the next place to bring under your notice some of the greatest achievements of the modern engineer in directing the great sources of power in nature in one special department of engineering, and the insignificant results accruing therefrom as compared with those arising from the development and exertion of analogous forces by nature herself. I shall then ask you to bear with me whilst I allude to the advanced position of engineering science, and then point out how prone we sometimes are to be so dazzled by the brilliancy of the light of that science as to be unable to see, or seeing refuse to believe, that some of those marvellous workers of bygone ages ever possessed a scintilla of scientific knowledge as we understand it, because the evidence thereof has not come down to us in the

shape of text-books or treatises, and because we cannot fathom, or do not understand, all the deeper principles underlying some of their most majestic creations. I shall then take you back with me towards the childhood of the world to see if we cannot discover in the long past the well-defined shadows of some of those things which we deem to be of essentially modern creation. I think we shall find that the principles underlying some of the most remarkable inventions and discoveries ascribed to our own times were by no means unknown to the ancients, and that in some instances the inventions themselves have been distinctly anticipated.

In considering the results accomplished by the modern engineer in directing the forces of nature, my experience with explosive compounds leads me to look at the question more particularly from this point of view. In our explosives we have one form of condensed or concentrated power, resulting from an ingenious and intimate admixture of all the elements which it is necessary should be united in combustion, including oxygen, so that in action they are independent of the atmosphere and will explode under water. Man thus by simple means collects, compresses, and stores up some of the forces of nature in a portable and handy form, and at the proper moment releases them and utilises their power by directing it against those formidable obstructions which it is sometimes found necessary to remove in the interests of material progress and civilisation. By the application of heat in one form or another, this stored-up power in a high state of tension is instantaneously set free and acts with a greater or less degree of intensity according to the nature and composition of the explosive. The most common forms of explosives are gunpowder, guncotton, and dynamite, combustion in the first being comparatively slow, and in the two last inconceivably rapid. It is this rapid development of intense power in dynamite and its congeners that renders it so useful to the engineer in removing large masses of rock in a short time and at a comparatively small cost. Although I have done a certain amount of blasting in ironstone mines and open workings, I cannot lay claim to having fired any very big charges. My heaviest blast was made with 115 lb. of lithofracteur, a species of dynamite, possessing about the same strength but exerting a greater rending power, and being a nitro-glycerine preparation. This was on the Jersey Harbour works, where Sir John Coode desired to remove amongst other obstructions a wall of rock which was standing out from the main rock, but running in a line with its face, there being a space of a few feet between the two. This wall was about 20 feet high, 19 feet long, and 12 feet thick, the foot being

exposed for a short time only at low water and the rock being nearly covered at high water, ordinary tides, thus affording the advantage of the resistance of a good head of water for tamping. At low tide I planted my 115-lb. charge in the angle formed by the wall and the main rock, with the capped fuse inserted, which I led up to the top of the rock. At high water, which happened to be at midnight, I rowed out, landed, lighted my fuse, rowed away, and in a short time heard the low rumble of the sub-aqueous explosion and the tumbling of the water. The next low tide revealed the wall of rock reduced to fragments and lying on the bottom, the explosion having also acted in a downward direction. It was computed by the engineer of the works and myself that at least 400 tons of rock had been dislodged by the blast. This gives about $3\frac{1}{2}$ tons per pound of explosive used, which may be taken as a fair average of what it should do.

In the autumn of 1879 I witnessed two heavy blasts with gunpowder at some granite quarries on the western shores of Loch Fyne, near Inverary. The first of these blasts was effected with 3 tons of gunpowder at the Crarae quarry, which is about 8 miles from Inverary, and the second with 5 tons at the Furnace quarry, which is some two miles nearer that town; both quarries belong to Mr. Sim. The quarries present extensive faces, and it is mainly the faces which are brought down, the powder being disposed in galleries to the rear of the face and as near as possible to the dividing joints of the rock. The mines were fired by electricity, the 3 tons of powder in the Crarae quarry bringing down an estimated aggregate of about 60,000 tons of granite and the 5 tons at Furnace about 100,000 tons. This gives in each case 10 tons of rock per pound of powder, but it is to be observed that the conditions of work were comparatively easy, the face only requiring a forward heave to detach and throw it down on to the floor of the quarry below.

There are two cases of heavy blasting with gunpowder on record, which I may here refer to in passing. The first of these was the removal of the Rounddown Cliff at Dover in 1843, in which operation 18,500 lb., or $9\frac{1}{4}$ tons, of gunpowder were disposed in three separate charges which were fired simultaneously by a voltaic battery. The second case was at Holyhead, when the harbour was being made. One of the heaviest blasts there was 12,000 lb., or 6 tons, of gunpowder, divided up into several charges and ignited simultaneously by means of a platinum wire heated by a Grove battery. The total quantity of rock stated to have been removed by this blast was 40,000 tons, or 3·33 tons per pound of explosive.

In the early part of last year a considerable blast was effected

with dynamite in a stone quarry at San Francisco. Four galleries were driven each 50 feet into the rock, and were returned at the end at right angles, forming a letter L. The explosive, of which 5½ tons were employed, was placed in the return galleries, the charges being tamped with débris. The four explosions were arranged to occur successively, the first to loosen the rock and make the work of the next easier, and so on with the second and third. It was estimated that the 11,000 lb. of explosive had displaced 35,000 tons of rock, being a little over 3 tons per pound, and thus practically agreeing with my own experience.

The heaviest known blast, however, I believe to be that which was successfully accomplished last autumn at the entrance to East River, New York, known as Hell Gate, when 150 tons of nitro-glycerine compounds were exploded simultaneously in removing Flood Rock. Nine years previously, namely in September 1876, the most extensive blasting operation up to that date was successfully accomplished in the demolition of Hallett's Reef at the same place. The area then operated upon was three acres, and the weight of nitro-glycerine preparations (chiefly dynamite) used was about 25 tons, the quantity of rock demolished being estimated at over 63,000 cubic yards. Assuming the rock to weigh 2 tons to the cubic yard (that was the weight of the rock in Jersey), we have 126,000 tons of rock removed by 50,000 lb. of explosive, or rather less than 3 tons per pound. After the removal of Hallett's Reef, operations were carried on at Flood Rock on the opposite side with a view to its removal, and by this means to render safe the navigation of the channel. On October 11th, 1885, this object was successfully accomplished by the aid of over 150 tons of explosives, which is six times as much as was used on Hallett's Reef, and which constitutes the biggest blasting operation on record. In this case, nine acres of rock were honeycombed, and charged with 75,000 lb. of No. 1 dynamite and 240,000 lb. of rackarock, which is one of the potash class of explosives. The nine acres of rock had been pierced from below with four miles of tunnelling in galleries, the floors of which were from 50 to 64 feet below mean low tide, with walls of from 10 to 24 feet thickness between them, and supported by 467 columns of rock, each 15 feet square. In the columns and rock roof of these galleries nearly 14,000 holes, of an average depth of 9 feet, had been drilled, and each was charged with a 6-lb. cartridge of rackarock and a 3-lb. cartridge of dynamite. These were all connected by a battery, so as to be exploded by a single electric spark. The shock of the explosion lasted about 40 seconds, the visible results being the raising of an enormous volume of water to a height of 200 feet into the air. Upon

subsiding, it developed a mass of broken rock, from which fire issued for a short time; but in two minutes more everything was quiet. No damage was occasioned to property, although a tremor caused by the explosion was felt at some distance. An examination of the rock showed that the explosion had been completely successful. General Newton, the chief engineer, states that it will probably cost half a million of dollars and take two years to remove the masses of broken rock. When the work is completed, the channel will be 1200 feet wide, instead of 600, as at present, and 26 feet deep, enabling ocean steamers to enter at all tides.

Such are some of the results produced by modern engineers with the aid of the most powerful explosives known. Great as they are, comparatively speaking, how utterly insignificant they appear when compared with the results of the development of some of the occult sources of power in nature as illustrated by that gigantic earth-displacement which took place on August 26th and 27th, 1883. I refer of course to the stupendous upheaval of Krakatoa on the coast of Java, the most formidable volcanic eruption recorded in modern times. It has been computed that the eruption and the tidal wave by which it was followed swept away at least 50,000 human beings at a stroke. It caused the entire disappearance of an island about 3000 feet in height, and transformed the geography of the region of the Sunda Straits. The usual volcanic products, including the finest particles both solid and vaporous, were ejected into the air to a height that no man will ever say, for it is on record that for many miles around the scene of these devastating forces noon was as black as night, and darkness was over all the land for from 36 to 40 hours. The scale on which the work was done was such that even the noise, the weakest part of it, was heard at a distance of 2000 miles. The shivering of the island produced a wave of water 100 feet high, which destroyed everything over which it swept, and left its mark on tidal registers nearly all over the world. The mere air pulse produced by the last fearful cataclysm was strong enough to pass with its gradually widening circle nearly three times round the globe. It is easy to imagine that with disruptive forces at work on this gigantic scale, millions of tons of matter and perhaps millions of cubic miles of vapour must have been hurled into the upper air. The coarser part of this matter would soon descend, as indeed was the experience of the crew of the British ship *Charles Ball*, Captain Watson, which vessel encountered the full force of the downpour of matter, being in the vicinity of the island of Krakatoa. The finely divided particles, however, doubtless remained in suspension, and were

probably the cause of the magnificent sunsets observed all over the world in the November and December following the eruption. Besides the sunsets, there was a peculiar appearance of the moon, which had at times a pale green tint, as noticed by myself and others. The remarkable sunsets of 1883 recurred in the autumn of 1884, although not by any means with such intensity and universality. These sun-glows were repeated in the autumn of last year, but with diminished frequency and brilliancy. They have served, however, to keep awake the discussion as to their real nature and cause, but whether they were due to volcanic or cosmical dust, or to some other undiscovered medium, is a point which has yet to be settled, if it ever can be. The theory that they were due to the Krakatoa eruption was not universally accepted at the time it was started; but in the early part of 1884 M. Angot, in a communication to the French Academy of Sciences, directed attention to a parallel case which occurred in 1831, when there was a violent volcanic eruption, and a new island was thrown up in the Sicilian sea, together with a quantity of ashes. For some time after the commencement of that outbreak, which lasted altogether for several months, brilliantly coloured sunsets were observable at many places on the continent.

A singular circumstance is on record in connection with the Krakatoa eruption, which is worth referring to here. On August 26th, 1883—the date of the vast upheaval—strange subterranean noises were heard in the island of Cayman-Brac, in the Caribbean Sea. This island is situated in 20° north latitude, and 80° west longitude, and to the south of Cuba. The inhabitants, who are tortoise fishers, were on that day alarmed by noises like the rolling of distant thunder, although the sky was perfectly clear. Seeing no signs of thunder, they imagined that some volcanic eruption was about to take place; but little by little the singular phenomenon diminished, and they ascertained it to be subterranean. Now the Cayman group are the antipodes of Java, where the eruption took place. The supposition, therefore, that these subterranean noises were due to the Krakatoa volcano at once suggests itself; the disturbance being propagated through the mass of the earth.

Notwithstanding the lamentable destruction of life attendant upon the Krakatoa eruption, it has not been without valuable results to science. Apart from the investigations of sunsets, it led to the correction of many remarkable data in the transmission both of air-waves and water-waves, and with respect to the true nature of volcanic action. A committee was appointed by the Royal Society to collect information as to the eruption, and they have got together a vast amount of information which

will doubtless be turned to good account in their report. I recently wrote to the chairman of the committee, Mr. G. J. Symons, respecting the probable date at which this report might be expected, and he informs me that the members are still hard at work, and that it will be some months before any report can be issued, their labours being so heavy and in many respects so difficult.

The earth disturbance which occurred in Essex in the early part of 1884, also yielded some valuable data to science. In fact, were it not for the disasters which these and similar phenomena entail upon humanity, they would be welcomed as important matters for scientific investigation.

But the Java catastrophe, frightful though it undoubtedly was, is not without parallel in the annals of seismological disturbance. So few of us carry the facts of history in our heads, and so many of us are liable to have our imaginations unduly heightened by such tragic visitations, that we are apt to describe every extraordinary occurrence as unprecedented. It may therefore prove interesting as well as instructive if I reproduce from the *Science Monthly* for January 1884 a few facts and figures connected with the subject. Taking the number of lives sacrificed as affording the most concise indication of the destructive effect of earthquakes in the past, the following approximate figures will help to reduce our most recent experiences to their proper proportions. Confining ourselves to the Christian era, we find that in the year 526, an earthquake in Italy destroyed about 120,000 lives. In 742 again, the greater part of Asia was severely shaken, more than 500 towns were wrecked, and an incalculable number of the inhabitants were killed. India, in 893, was the scene of an equally terrible shock, when, it is said, about 180,000 lives were lost. In 1040, Tabriz (Persia) was completely demolished, and 50,000 of its inhabitants swallowed up in the ruins. In 1137 one of the earliest of the dreadful Sicilian earthquakes occurred, Catania being overthrown, and 15,000 persons killed. Some thirty-two years later (1169) a similar number perished in Sicily and Calabria. Almost exactly a century afterwards (1268), Cilicia in Asia Minor suffered a tremendous shaking, over 60,000 lives being sacrificed. An interval of nearly two centuries now elapsed, the next notable earthquake occurring in 1456, 40,000 of the people of Naples being destroyed by it. Lisbon, in 1531, was almost reduced to ruins, and about 30,000 of its citizens entombed. The following century was remarkable for several most destructive shocks. One occurred at Naples in 1629, when the destruction of life was set down at 70,000 souls; another took place in Russia (1667), the victims numbering 80,000; and the most

terrible of all happened in Sicily (1693), more than 100,000 persons perishing.

The eighteenth century opens with the destruction of Yeddo, in Japan, and 200,000 of its inhabitants (1703). Only three years later, Naples suffered another of its many shocks, 15,000 perishing on this occasion. Algiers was the scene of several earthquakes in 1616; victims, 20,000. In 1727, Tabriz was again demolished, together with 77,000 persons. The city of Pekin was levelled with the ground in 1731, about 100,000 Celestials being killed. In 1755 occurred the celebrated Lisbon earthquake, which seems to have marked a period of great seismological activity, shocks being frequent and severe in all the volcanic regions of the globe about that year. The total number of persons who perished between 1754 and 1756 must have been hundreds of thousands. A calm followed the storm, for the latter half of the century was comparatively free from disturbance. 53,000 lives were destroyed in Japan in 1793, and shortly after, when the city of Quito, in Peru, was overthrown, 40,000 lives were lost. Coming nearer our own time, we find a destruction of 26,000 lives in Italy in 1805; 12,000 in Venezuela (1812); 20,000 at Aleppo (1822); 7000 at Algiers (1825); 10,000 in St Domingo (1842); 14,000 in Italy (1851); 10,000 in Persia (1853); 30,000 at Yeddo (1855); 15,000 in the Argentine Republic (1861); 10,000 in the Philippine Islands (1863); 25,000 in Central America (1868); and 14,000 in Chili (1875).

As I have already stated, these figures are only approximate, and in fact the whole record is somewhat barren, and affords but a poor idea of the destructive earthquakes of history. It has been computed that at least 7000 shocks must have been experienced within historic times. Many of these have destroyed, not thousands merely, but tens of thousands of human beings; hence the actual victims in all ages must be many, many millions. But the point here is that the least of these eruptions utterly eclipses anything in the way of earth-dislodgment ever attempted by man.

Regrettable, however, as are such occurrences as those to which I have just referred, they carry with them some consolation. They may even be said to have their bright side, inasmuch as they indicate that our earth still possesses life and vigour. In this respect the earth presents a marked contrast to the moon, some fine photographs of which I saw a short time since, and which presented it to the eye as the embodiment of sterility and desolation—a magnificent spherical clinker. Science has revealed to us the fact that the moon is absolutely dead; that it has not sufficient life left to ooze forth the tiniest mud volcano or to spurt out the feeblest geyser; that no throb

ever reaches its surface, and that not the faintest rumble is ever echoed from its jagged mountain sides. No earthquake wave can ever sweep its face, for earthquakes there are things of the almost infinite past; the last drop of water quitted its valleys ages ago, and its very atmosphere has deserted it. Not even the 600° heat of the sun, which beats upon every part of the moon for a fortnight at a time, is able to quicken into life one atom of its mass, which, according to Professor Langley, has a temperature lower than that of melting ice. Everything goes to show that at one time its volcanic activity must have been of the most stupendous kind, far exceeding anything yet witnessed on this earth. But that has gone with its water and its atmosphere, and it is difficult to realise the fact that when we gaze upon its effulgent loveliness we look upon what is physically speaking a charred and blackened corpse. That the earth has not yet lost its vigour is proved by the Java catastrophe and by subsequent similar minor manifestations. Moreover, we are told that so long as the old earth is equal to efforts of this kind, we need not fear its end; but that when volcanoes and earthquakes cease from off its face, the end may be within almost measurable distance. Much of its ocean water will have been absorbed by the underlying rocks, and the atmosphere may be rarer and less life-supporting than it is now, until at length, with the last atom of internal heat, the last drop of water and last particle of air, all life will have vanished and a dead earth will shine upon a dead moon.

I next proceed to notice the present advanced position of the science of engineering—which lays under contribution so many other sciences—and of our own profession which puts that science into practice. It is admitted on all hands to be a great science and a noble profession. It has compassed mighty purposes, has accomplished great ends, and, I cannot doubt, is destined to accomplish greater yet. Born, as it has been, of the present century, in fifty years the engineering profession has practically overrun the face of the whole earth, materially altering its physical appearance and condition. Seas have been united, countries have been divided, and continents have been spanned by the engineer, whilst lasting monuments of his power and skill dot the sea as well as the land. It is only repeating a truism to say that fifty years have produced great and marvellous changes in almost everything that pertains to life, thanks to the engineer. Even life itself is now due to conditions which did not exist fifty years ago. Where, at that time, disease was rife and in many cases death inevitably stepped in and prematurely claimed its prey, the sanitary engineer has

successfully intervened and has removed the cause and prevented the effect. Where, at that time, months were occupied in transmitting news to the other side of the globe, seconds now suffice with the aid of the electrical engineer to flash intelligence to and from the uttermost parts of the earth. Where, at that time, it took the lumbering old stage-coach days to accomplish a journey to the north at an average speed of 10 miles an hour, the locomotive, thanks to the mechanical engineer, now takes us the same journey in a few hours at a speed of from 50 to 60 miles an hour; and lastly, where at that time a few hundred copies of a newspaper were all that at the best could be produced in an hour, and those on detached sheets of paper, in the present day, thanks to Mr. Walter of the *Times* and also to the mechanical engineer, they are printed at the rate of thousands per hour, the paper being continuous and miles in length. In all this there is no idle boasting, it is but a simple statement of incontrovertible fact. And if we stopped at a simple statement of fact, I might have nothing more to say upon the subject. But do we always so stop? Is there not rather an occasional tendency both with writers and speakers to magnify our times at the expense of former ages? I think there is. Not many years since a prominent member of the profession expressed his regret at the non-existence of engineers 500 years ago. If he meant engineers as members of a recognised professional body, engineers of the present type, men who construct railways, manufacture steam-engines and machinery, and build ironclads and steel ships, his regret is intelligible. But even then, honestly speaking, he should have little cause for regret, inasmuch as the whole available surface of the earth might long since have been so completely engineered as to have left him no opportunity to have distinguished himself. He might then have wished that there had been no engineers 500 years ago. But were there no engineers at that time? To take one instance only, one of the most important branches of mechanical engineering in the present day is that relating to the manufacture of artillery. It is a matter of history that cannon were first used in warfare at the battle of Cressy, which took place in 1346, and by the Moors in the defence of Algeziras about the same period. Surely the manufacture of artillery was relatively of as great importance in the reign of Edward III. as in that of Queen Victoria, and those who carried on that manufacture are as much entitled to be considered engineers as those who manufacture the 100-ton guns of the present day. Perhaps more so, if we judge them by their light and take into consideration the disadvantage at which they were placed as compared with their successors in the craft, who, with all their ingenuity

and experience, and with all the materials and machinery at their command, manage to have a gun burst accidentally now and again. I know it may be urged that the guns of old were very small weapons. There is, however, one example still extant, of a more recent date it is true, but of somewhat remarkable size and construction for the period at which it was produced. I refer to the celebrated Mons Meg in Edinburgh Castle. This gun is said by some to have been manufactured at Mons in Belgium in 1476, whilst others hold that it was made in Galloway and was used by James II. at the siege of Thrieve Castle in 1455. It is, however, known to have been employed at the siege of Dumbarton in 1489, and at that of Norham Castle in 1497. It burst when firing a royal salute in 1682. This gun is 13 feet long by 20 inches in diameter, and is formed of longitudinal bars of wrought iron encircled by hoops of the same material, its weight being 5 tons.

As a matter of fact, however, the "five hundred years ago" was merely an arbitrary definition of time on the speaker's part, for his remarks were intended to apply to the long past in general, and to imply that engineers were a race of men unknown to any time but the nineteenth century. The expression was in fact begotten of the spirit which, as I have observed, would fain magnify the present to the depreciation of the past. When we indulge in this view, we surely forget the broken arches of the huge aqueducts and bridges which cross the Campagna, and which ought to remind us of the engineers whose names history has failed to preserve. Those who have forded rivers by the Roman paved paths, or who have driven for miles along the firm Prætorian Ways which, two thousand years ago, the conquerors of the world constructed for the march of their legions, might even doubt whether Telford, Macadam, and their successors have made any very great advance on their predecessors in the art of road making. The *Cloaca maxima* was a sanitary institution which we have yet to imitate. The palaces and sewage arrangements of Nimroud, the hanging gardens of Nebuchadnezzar, the canal which Xerxes cut across the isthmus of Mount Athos, were, like the bridge of boats with which he spanned the Dardanelles, engineering works which we should all do well not to utterly ignore when we would exalt ourselves and our times in these respects. The men who reared the pyramids and who erected the monoliths of Egypt possessed an art which died with them. It is, moreover, not a little singular that in the past, nations which are sometimes credited with having possessed no knowledge of science have produced the most beautiful structures. The Mahommedans and the Hindoos, ages ago, without text-books, so far as we can learn,

without scientific schools or rules to guide them, stumbled into the creation of those charming edifices which it is one of the privileges of Eastern travellers to see. That they were guided in some sort by scientific principles we cannot doubt, and although we may not be able to discover them, are we justified in ignoring them? And how far back into the remote past can we peer in the vain hope of catching a glimpse of these workers at their early labours? Where is the page of history that discloses to us —

> "A race who carved the temple of the sun,
> And mighty fanes from out the living rock,
> Fanes which lay mouldering o'erthrown by time,
> While yet the pyramids were not!"

We may exhaust tradition and still find ourselves no nearer the time when such science as they had was flourishing at its best. The stupendous cavern temples of Nubia and Upper Egypt proclaim an antiquity in the contemplation of which all tradition is lost sight of. The researches of some of our modern explorers lead to the conclusion that the most ancient buildings of the Thebaid, especially the great temple of Karnac, which had a circuit of a mile and a half, are to a large extent constructed of the materials of edifices themselves as ancient when they were thus dismantled, as those which are now standing have become since their demolition. Thus we have come to be aware that the antiquity of a civilisation so remote that tradition scarcely reaches to the latest epochs which its monuments illustrate, is at least doubled by the evidence of those monuments themselves.

And here I may refer to the fact that early in 1884 fresh proofs of the existence of an ancient civilisation in Mexico were discovered in Sonora about 60 miles south-east of the town of Madeleine, where some explorers found in the heart of a virgin forest a pyramid, which is 4350 feet round the base and 750 feet high, that is, nearly double the size of the great pyramid of Cheops. From the base to the summit there is a roadway on which vehicles can travel round the vast erection in a spiral. The outside walls are built of granite blocks carefully tooled and bedded. A little further off is a hillock, with hundreds of caverns or chambers cut in it, from 5 to 15 feet wide and 10 to 15 feet long. They have no windows, and are entered by the roof. The walls are covered with hieroglyphics and curious pictures, with the feet and hands of men. Stone utensils have also been found there. Who the builders of these ancient monuments were is still unsettled, but they probably belonged to the Mayos, who formerly inhabited Sonora,

and were a different race to the Indians, having blue eyes, a white skin, and blonde hair.

The key-note to the favourable results of a comparison instituted between the works of the ancient and the modern engineer as against the former is undoubtedly scientific progress and development in its broadest and fullest sense. The bridges with which Robert Stephenson spanned the St. Lawrence and the Menai Straits are, as efforts of skill, infinitely beyond anything that Roman perseverance could accomplish. In like manner the simple contrivance by which George Stephenson succeeded in carrying the Liverpool and Manchester railway across Chat Moss, manifests a genius for a corresponding display of which in the works of the ancients we search in vain. Their works astonish us by their vastness, by their magnificence, and by the expenditure of brute force which must have accompanied their construction. Those of the moderns aim at introducing lightness and elegance of design and economising material and power, and they have to be examined in detail before their full merits are apparent. Take the magnificent bridge over the Firth of Forth, the works of which I recently had the opportunity of inspecting, and which will twice span 1700 feet of sea at a stretch, at a height of 150 feet above high-water level, with its cantilevers towering 200 feet higher, and examine its details. In their minuteness and relative correctness with regard to each other and the whole structure, they are such as never could have been conceived by the ancients. Size and massiveness with them went hand in hand, resulting in an appearance of grandeur typified rather by the Egyptian style of architecture than by the modern Gothic. We should endeavour so to combine size with lightness, as to produce an appearance of elegance. Art and science, beauty and utility, when rightly understood, are never in conflict, and they can be as perfectly in agreement in a chimney-stack in Manchester as in the beautiful Gothic of Milan Cathedral. The happy blending of true æsthetic principles with the more rigid and circumscribed requirements of utility in constructive art, cannot fail to produce what that gifted writer Von Schlegel has so beautifully described as "frozen music."

But if we reduce the design of the Forth bridge to first principles, what do we find? Simply that it is a development—a very grand one it is true—of a rude kind of structure raised by the inhabitants of Thibet, Cashmere, and the mountains of India, consisting of beams projecting from piers. Each beam is made to project beyond the one that supports it until the two sides approach sufficiently near to admit of a beam being laid across to connect them. Here we have a

rude plan which has been converted into a scientific and beautiful method of construction by engineering science. The old semaphore system of signalling which for years many of us were accustomed to see at work on the roof of the Admiralty at Whitehall and elsewhere, was of more imposing appearance than a thread of telegraph wire with a Morse sending instrument at one end and a Wheatstone automatic receiver at the other. But how incomparably superior to the former is the latter! Again, look at our glorious Nelson's ship the *Victory* in Portsmouth Harbour, and compare her with a modern torpedo boat, steaming past her at 22 knots an hour.* The contrast in size is striking, but the contrast in power in proportion to size is simply startling. The one would hardly make a cock-boat for the other, although, whilst the ancient engine of war was changing her course but a few points, the modern machine could come upon her, deliver its deadly message, and steam well clear of the vortex caused by the sinking three-decker.

I have said that the key-note to the advanced position of the present age is scientific progress, and I now desire to direct your attention to the circumstance that the principles involved in and underlying several of the most recent developments of science which constitute that progress were not unknown to some who lived in bygone ages. But although they appear to have been conversant with those principles, they lacked the power of giving practical effect to their knowledge. It is the possession of this power—this civilising quality—and its legitimate utilisation which have led to the remarkable developments of modern times. The scant records of the past have for the most part only brought down to us the theories of the ancients in such a vague form as to cause them to be passed by as the utterances of visionaries or the fantastic day-dreams of poets, unless perchance a train of thought should establish a connection between what may be regarded as the prophecy and its fulfilment. Take, for example, electrical telegraphy, which forms one of the practical adaptations of that latest born daughter of modern science—electricity. Many of us are in the habit of unthinkingly associating with its invention, the honoured names of Cooke and Wheatstone, so prominent is their position in this connection, and so important the part they played. Great, however, as was their work, they only took up and perfected that of many others, their predecessors, whose labours led up to their

* In the speed-trials of two torpedo-boats recently built by Messrs. Yarrow and Co., when the boats were loaded with 17 tons (representing the fighting trim), the speed obtained was 22¼ knots an hour, and when tried on the light water-line 24 knots, which is equal to 27½ statute miles per hour, being the highest speed on record.

victory. The electric telegraph had, properly speaking, no inventor, it grew up little by little, each inventor contributing his share towards its advancement. At least a score of names adorn the roll of fame in this respect. Commencing very nearly a century ago with those of Galvani and Volta, we encounter in our run down the stream of time, the names of Sömmering; Schweigger; Coxe; Romagnési; Oerstadt; Ampère; La Place; Fechner; Alexander; Sturgeon; von Cannstedt; Faraday; Morse, Gauss, Weber, and Steinheil, each of whom contributed successively a round in the ladder, which was ascended by Cooke and Wheatstone. These latter, however, were the first to establish a telegraph for practical purposes on a comparatively large scale, and thus their names came into prominence, whilst those of Ampère, von Cannstedt, Steinheil, and others remain comparatively unknown in this immediate connection.

In referring to Galvani and Volta, I have looked back a hundred years to what appears to be the first conception of the idea of an electric telegraph. The means of attaining this end, however, may have been struggling for birth in reflective minds for many centuries past, for aught we know. In support of this, let me go back two centuries and a half, to the time of Galileo, who appears to have been fully aware of the importance of employing magnets for transmitting intelligence to a distance, although he did not see his way to the practical accomplishment of his idea. In one of the dialogues which he wrote in 1632 on the two great rival astronomical systems, he makes one of his myths, Sagredus, to say, "You remind me of one who offered to sell me a secret art, by which, through the attraction of a certain magnet needle, it would be possible to converse across a space of two or three thousand miles. I said to him that I would willingly become the purchaser, provided only that I might first make a trial of the art, and that it would be sufficient for the purpose if I were to place myself in one corner of the sofa and he in the other. He replied that in so short a distance the action would be scarcely discernible; so I dismissed the fellow, and said that it was not convenient for me just then to travel into Egypt or Muscovy, for the purpose of trying the experiment, but that if he chose to go there himself I would remain in Venice and attend to the rest." * Whether the idea originated with Galileo or with another who communicated it to him is not certain; but what seems perfectly clear is that in this dialogue we have the first absolutely defined conception of the electric telegraph. This interesting fact was pointed out to me some years since by an old friend, Mr. Richard Bellamy, a gentleman

* 'Galilei Systema Cosmicum,' Dialogue I., Leyden Edition, 1700.

of considerable erudition. He also communicated it to Mr. Robert Sabine, who introduced it in the preface to his 'History of the Electric Telegraph,' in Weale's Rudimentary Series. Again, take the case of lightning conductors. Here we find that Franklin was probably anticipated in his discovery of lightning conduction. According to M. de Rochas, the ancient Etruscans understood the art of guiding lightning. Servius relates that in ancient times the priests ignited their sacrifices by lightning, and upon one occasion Tullius Hostilius was struck dead, because he neglected the precautions laid down by Numa.

Turning to surgical science, let us take the circulation of the blood. The discovery of this wonderful principle was undoubtedly enunciated by Harvey in the early part of the seventeenth century, namely in the year 1619. But I have read a very striking description of the circulation of the blood in a book which was written 2600 years before Dr. Harvey's time. It is possible that Harvey may have read it too, and that, as a medical man, it may have suggested to his mind a research in the direction in which he eventually made his great discovery. It is also possible that, like thousands upon thousands of others, he may have merely read it, and have considered it a piece of poetic imagery bearing some occult meaning known only to the writer and never further developed by him. In fact, like Wordsworth's Peter Bell—

> "A primrose by a river's brim
> A yellow primrose was to him,
> And it was nothing more."

I refer to that beautiful description of the decay, by reason of age, of man's natural powers, written by Solomon at the close of the Book of Ecclesiastes in the year 977 B.C., or nearly three thousand years ago. He describes the dimming of the various faculties under exceedingly poetical but equally appropriate and unmistakable symbols, and he goes on to say: "Or ever the silver cord be loosed, or the golden bowl be broken, or the pitcher be broken at the fountain, or the wheel broken at the cistern." * Here then we have the silver cord—the spinal marrow, the golden bowl—the heart, the pitcher at the fountain and the wheel at the cistern—both representative of the means by which the Easterns obtained and still do obtain water, and in conjunction with the golden bowl, clearly symbolising the circulation of the blood.†

* Ecclesiastes xii. 6.

† The "Sakia" or Persian wheel, or the "na'ura" as it is called in some countries, is the great instrument of irrigation in the East. It consists of an

Another cognate matter is the comparatively recent discovery of those minute entities known as bacteria. I fear we can only call this a re-discovery, inasmuch as two centuries ago the celebrated naturalist Leeuwenhoek, with wonderful skill, considering the very imperfect instruments of his time, anticipated modern physicists in this connection. The discovery of this circumstance is due to Professor E. Cohn, of the University of Breslau, who, a year ago, in a communication published in the *Allgemeine Handelsblatt*, recapitulates the substance of a correspondence of Leeuwenhoek with Francis Aston, of London, a member of the Royal Society. Leeuwenhoek, writing from Delft, in 1683, reports that among the débris of food remaining between his teeth he had discovered with the aid of the microscope living organisms moving with great activity. He distinguishes various kinds among them, which he describes so precisely that they are easily recognisable. One, which occurs least frequently, resembles a rod, the bacillus; others, twisting in curves, are bacteria; a third kind, creeping in snake fashion, is the vibrio ugula; another kind, of extreme minuteness, resembles a swarm of flies rolled up in a ball, and is evidently the micrococcus; its movements cannot be traced with certainty. He says that this species seems to be made up of parallel threads, varying in length, and remaining immovable, while other specks move in and out through the web. Leeuwenhoek marvels that these things could live in his mouth, notwithstanding his systematic habit of cleansing it. He instituted observations which showed that they were also to be found in the mouths of other persons. Some years later he could not discover any traces of those minute organisms, and he was led to attribute their disappearance to the use of hot coffee. But shortly afterwards he re-discovered them as lively as ever. In September 1692 he sent some sketches of them to the Royal Society. From this correspondence it would seem that the knowledge concerning bacteria made no advance for nearly two centuries, but it clearly places Leeuwenhoek in the position of their original discoverer.

Quitting medical science, let me turn to that which relates to the production of our modern weapons of war and our most cunning devices for endeavouring to deprive our enemies of the pleasures of conquest. Here we have one who was both poet and seer, most distinctly foreshadowing the submarine travelling

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endless chain formed of twigs and strung with earthen pitchers. The chain passes over a wheel driven by oxen, and the pitchers raise the water from the well and deliver it into a cistern.

torpedo. This was Ben Jonson, who, in his play 'The Staple of News,' wrote, in act iii. scene 1, as follows:—

"THOMAS. They write here one Cornelius' son
Hath made the Hollanders an invisible eel
To swim the Haven at Dunkirk and sink all
The shipping there.
PENNYBOY. But how is't done?
CYMBAL. I'll show you, sir.
It's an automa, runs under water
With a snug nose, and has a nimble tail
Made like an auger, with which tail she wriggles
Betwixt the costs of a ship and sinks it straight.
PENNYBOY. A most brave device
To murder their flat bottoms!"

What therefore to the playgoers of Queen Elizabeth's time was the most palpable absurdity a poet's fancy could invent, is to us simply an accomplished scientific fact. In like manner Dean Swift 160 years ago credited the astronomers of Laputa with the discovery of two satellites revolving about Mars, whilst the actual discovery of Mars's moons did not take place until about the year 1877.* These are, to say the least, curious coincidences, and however much Ben Jonson's "invisible eel" may have been the offspring of poetical fancy, and however little Swift's moons may owe their origin to astronomical knowledge, the fact remains that we have two distinct scientific forecasts which have been singularly verified in our own times. Even more remarkable than either of the foregoing is the case of the poet William Drummond of Hawthornden, who 260 years since did not simply shadow forth at the bidding of the muse what might then appear an absurdity or a quaint conceit, but in very precise language indicated some of the most important weapons of war used by the armies and navies of the civilised world in the present day. Looking through the records of the Patent Office some years since for information upon a cognate subject, I came across a specification entitled "The Letter of Master William Drummond for the Construction of Machines, Weapons, and Engines of War for Attack or Defense by Land or Sea, &c.," and dated September 29th, 1626. In this remarkable document no fewer than sixteen inventions are apparently included, although only fifteen are indicated. The first is de-

* "They have likewise discovered two lesser stars or satellites which revolve about Mars, whereof the innermost is distant from the center of the primary planet exactly three of his diameters, and the outermost five; the former revolves in the space of ten hours, and the latter in twenty-one and an half; so that the squares of their periodical times are very near in the same proportion with the cubes of their distance from the center of Mars, which evidently shows them to be governed by the same law of gravitation, that influences the other heavenly bodies."—*A Voyage to Laputa*, &c., edit. 1755.

scribed as an instrument for cavalry use, by which a single man can be of no less avail in battle than five or six with the ordinary weapons. It is called a "box-pistoll" and a "muskett-box," and its effect is said to be at once terrible and rapid. This invention appears to answer to the repeating rifles and revolvers of our time. The second invention is described as a "shooting-spear" for a musketeer, and was probably a blade attached to the musket, as is the bayonet in the present day. The third invention is a number of musket barrels fastened together and called a "lightning chariot," which would appear to be the prototype of the mitrailleur and Gatling gun of modern warfare. The fourth invention is described as one from which three, four, or even five balls were to be fired in the same time as a single ball, and from the name given to this appliance of "open ordinance" it would seem that modern breech-loading artillery was foreshadowed.

The sixth and seventh inventions relate to guns, one of which is called a "flatscourer" and the other a "cutter," the inference being that the use of chain shot was here intended. The seventh invention is a movable fort or turret for soldiers. The eighth invention is omitted from the original document, nor is there any clue as to its nature. Now it is just possible that this missing invention may have been the one imported by Ben Jonson into his play, and I will give you my reasons for thinking so. I find that Jonson visited Drummond at Hawthornden in the spring of 1619, and the latter may then have communicated his ideas in this respect to the former. Jonson's wit would readily transform the more sober notion of Drummond into satire, and this might prevent Drummond with his more sensitive nature from making public the original of Jonson's ludicrous automaton. As tending somewhat to prove the correctness of my assumption, I would remark that the ninth is in some sort a cognate invention with Jonson's "automa," inasmuch as it is described as "a new sort of ship which can enter ports of any sort even barricaded by powerful chains, bars, or engines, and can destroy whole ships by fire or forcibly capture them. The ship," continues the specification, "from its immense and really terrible effect and its tremendous destruction of ports and ships, deserves to be called 'Leviathan.'" This would appear to embody the idea of our modern ships of the ram class.

Here Drummond appears to have exhausted his stock of invention as regards the arts of war, and he turns his attention to those of peace, for the tenth item is an instrument for measuring the speed of the wind or of a ship, in fact an anemometer and a log combined. The eleventh invention is a "sea postilion,"

which in some respects answers to our little steam launches. The twelfth invention is worthy of note; it is called a "lenth-compas, by which the distance of a voyage may be exactly calculated, and the different longitudes either at sea or on the nearest shore determined." Now here we apparently have the first suggestion of a means of discovering the longitude of ships at sea. The patent, as I have said, is dated 1626, and it was not until 1674 that a Frenchman—St. Pierre—submitted to Charles II. a method of effecting this object. The proposition was referred to a committee of astronomers, of which John Flamsteed—the first astronomer royal of England—was a member. Flamsteed drew attention to the incorrectness of the lunar tables by which the position of the moon amongst the fixed stars was to be calculated. Struck by the deficiency, the monarch is said to have at once founded the Observatory at Greenwich, which was called Flamsteed House. The king appointed Flamsteed to the post of astronomer royal at a salary of 100*l.* per annum. It does not appear that previously to this any attempt had ever been made to determine the longitude of a ship while at sea. To return to Drummond, I have only to notice that his thirteenth invention was a still for rendering salt water potable, whilst the fourteenth and fifteenth have reference respectively to burning-glasses and telescopes, or "lynxes eyes" as he calls them. In the sixteenth and last invention, Master Drummond falls foul of that *pons asinorum* of inventors of all times—perpetual motion—for he describes it as "a machine producing perpetual motion from a natural cause, and one incapable of fatigue, by the employment of whose aid a variety of mechanical operations may obtain motive power, and it is called momet." Such is the poet's remarkable list of inventions, which I need hardly say embraces a far greater variety of subjects than could be coaxed into a single patent in the present day, especially under the new law. The inventions are not in any case described in detail, nor does Drummond illustrate them, a course which some inventors of our own time would like to be permitted to pursue. Neither am I aware of Drummond having carried any of his inventions into practical effect. Of course, breech-loading arms are by no means so modern an invention as is generally supposed, as the relics in the Tower of London show. Possibly some of these might be traced to Drummond's patent, although this is very problematical. Be this as it may, there is no question that those of his inventions which relate to the arts of war do certainly foreshadow many of those at present in prominent use, and which are regarded more or less as distinct creations of our own times. As it may occur to some that they

have come across this somewhat singular disclosure respecting Drummond before, I may here mention that the matter formed the subject of an article from my pen which appeared in the leading journal some nine years since.*

Before quitting the region of prophecy and fulfilment, I would observe that England's greatest poet—Shakespeare—appears to have placed himself in the same category as Jonson and Drummond. In the second scene of the fourth act of 'Troilus and Cressida,' Shakespeare makes Cressida say—

> "But the strong base and building of my love
> Is as the very centre of the earth,
> Drawing all things to it."

To my mind the poet here most clearly anticipates Sir Isaac Newton's great discovery of gravitation.

I have taken you back with me a long way into the past, not merely to pre-historic times, but I have even exhausted tradition. It must not, however, be assumed from this that I live in the past. I live in the present and for the future. I obey the poet's behest which is to—

> "Look back upon the past, and by the lights
> Which now are thine, onward towards the future."

Let us see, then, what the facts I have adduced teach us. I think they show us that what was aforetime spoken hieroglyphically and in fable is now being spoken plainly. In fact, the idealism, the imagination of a bygone age has in more instances than one become transformed into a reality in the present one. The dream of the poet, and the inchoate indefinite idea of the sage of antiquity, have become materialised and produce the daily bread of the artisan. That which was once purely spiritualistic, and which formed one of the highest elements of fiction —communication and identity of thought without sense of distance—has become strangely realistic, and is inextricably interwoven with the web of ordinary every-day life. It is, however, one thing to have a vision, but quite another to be able to give it form; one thing to conceive, but quite another to invest the conception with practical existence. But in the past, when knowledge was power of an oppressive and tyrannical character, a veil of impenetrable mystery was drawn around the early germinations of science by her bigoted votaries. The pursuit of knowledge was only permitted to the few, whilst the mass wondered ignorantly on in mingled terror and admiration. Hence it is reasonable to suppose that there were in

* *The Times*, November 29th, 1877.

former ages developments of science which ran parallel with some of our own—in fact, I have to some extent shown this to have been so—but they were most jealously guarded, and have been buried with overthrown nations. We may also learn a lesson of patience and unresting, but not restless, perseverance in our work. Great results are not achieved in a day, or, as Smiles appropriately puts it, "great inventions are not brought forth at a heat." They are begun by one, improved by another, and perfected by a whole host of successors.

The facts I have brought before you also point to the moral and material progress of the world. "The bee that hummed its busy hour through the bowers of paradise," wrote Sydney Smith, "fashioned its hexagon with the same mathematical precision which it does now and here. Six thousand years have added nothing to the sagacity of the horse, or the intelligence of the dog." But how widely different with man! He commences as a fire-worshipper and rises to a Newton, a Faraday, a Stephenson, a Siemens. Tempts the river in a few fragments of bark lashed together with thongs of raw hide, and crosses the Atlantic in an iron steamship of 22,500 tons burthen.* Burrows in the earth, and then builds a city with four millions and a half of inhabitants. Sticks a dried reed in a lump of fat to light his mud hut, and carbonises 2,200,659 tons of coal per annum to illuminate London.† Takes weeks to send messages on sticks to Montezuma from the coast, and at last reports in London the details of a battle fought in the Soudan the same morning. Slays his foe with a sling and a pebble chosen from the brook, and meets the enemy with a machine gun firing 600 rounds a minute by means of its own recoil.‡ Lays siege to a city with a balista, throwing a fragment of rock, and finally attacks a fort with a gun weighing 110 tons projecting a steel shell of 1800 lb., with a charge of 900 lb. of gunpowder. The axe-head that floated for a few seconds on the Jordan three thousand years ago, when "the iron did swim," was a miracle indeed. But what of the hundreds of thousands of tons of iron that are now continually swimming over our seas in masses some thousands of tons each?

These are extremes, and they stand very wide apart. But they are to-day the beginnings and the endings of science, and thus stated they bring out in bold relief the contour lines of progress. But they are the endings of science as regards

* The *Great Eastern.*

† During the year 1884 the Chartered Gas Company carbonised 1,568,821 tons of coal; the South Metropolitan Gas Company 457,637 tons, and the Commercial Gas Company 174,201 tons = 2,200,659 tons.

‡ The Maxim Gun.

the present only. They are by no means final, for science never stands still. They are but the landmarks of our times, which, as Emerson puts it, are "trivial to the dull, tokens of noble and majestic agents to the wise; the receptacle in which the past leaves its history; the quarry out of which the genius of to-day is building up the future."

The progress of science, however, is very far indeed from being measured or measurable by its material achievements. It has done infinitely more than merely give us railways, telegraphs, arms of precision, and other practical applications of its discoveries. What it has further done I cannot express better than in the words of a recent anonymous writer, who says:—"It has given to the human race, for the first time, standards of truth which are at once absolute and accessible, and has thus caused a knowledge of truth, of its tests and evidences, and an engrossing love of it, to become the chief mental characteristics of those who are really engaged in it. Great as have been the material rewards of science, the moral rewards will be greater still." The opportunities of scientific education, which are daily increasing in our midst, must hasten the coming of the time when these moral rewards shall be within the reach of all who will stretch forth the hand to grasp them.

March 1st, 1886.

PERRY F. NURSEY, PRESIDENT, IN THE CHAIR.

THE ROORKEE HYDRAULIC EXPERIMENTS.

BY E. S. BELLASIS, ASSOC. M. INST. C.E.

AN extensive series of experiments on the flow of water in large open channels was carried out on the Ganges Canal, near Roorkee, in Northern India, by Major Allan Cunningham, R.E., in the years 1874–79. The principal objects were to discover a good system of discharge measurement for large canals, and to test the applicability of the coefficients at present in use for calculating velocities. A full account of the experiments has been published,* and an abridged account is given in a paper read by Major Cunningham, at the Institution of Civil Engineers.† A discussion and correspondence on that paper took place, but there are still many important questions, whether affecting the suitability of the procedure adopted in the experiments, the accuracy and utility of the results obtained, or the correctness of the conclusions arrived at, which have not been touched upon, or have not been fully discussed. It therefore seems desirable that the subject should receive further consideration, and with this view the present paper has been written.

It is divided into three parts.

I. Instruments for measuring velocities.
II. Velocity-curves.
III. Discharges and coefficients.

I.—INSTRUMENTS FOR MEASURING VELOCITIES.

Surface-Floats.—The surface velocities were measured by surface-floats, consisting of small discs of pine or cork. These floats do not appear to have been very suitable, as the parts exposed to wind must have been too large in proportion to the submerged parts. There were, however, very few instances in which there was much up-stream or down-stream wind during the observation of the surface velocities, and the errors due to

* 'Roorkee Hydraulic Experiments,' E. & F. N. Spon.
† Min. Proc. Inst. C.E., vol. lxxi.

wind were to a great extent eliminated in the mean results by the combination of different "sets" of observations, in which the wind effects more or less neutralised each other. The surface velocities obtained may therefore be accepted as substantially correct.

Double Floats.—With the sub-surface velocities the case is very different. The sub-surface work consisted chiefly in the observation of velocities at each foot of depth on different verticals in the cross section of the stream. These velocities were all obtained by the double float of the ordinary pattern, that is, the pattern in which the upper float is made very small as compared to the lower float. The objections to which this kind of float is liable are, in effect, three, namely:

(1) Current-action on connector and upper float.—This causes the instrument-velocity to differ from the current-velocity at the level of the lower float. (See Figs. 1 and 2.)

Fig. 1.

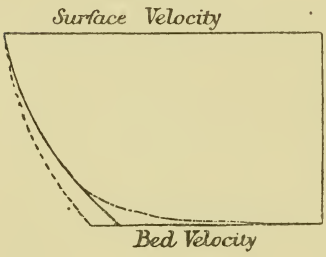
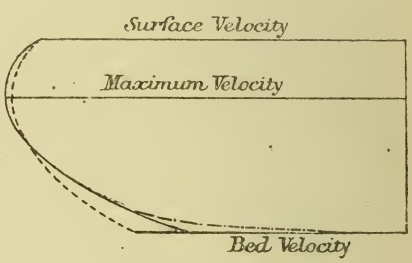


Fig. 2.



(2) Lift of lower float.—The connector assumes a curved and inclined position which depends on the form of the velocity-curve, and the lower float moves at a mean depth which is less than the assumed depth or length of connector.

(3) Instability of lower float.—Owing to the unsteady motion of the water, the form of the velocity-curve is constantly changing, and with it the lift of the lower float, but the instability is caused chiefly by the eddying masses of water which, particularly in a rapid stream, are constantly rising up from the bed.

There may also be lateral deviation of the lower float, but this is not of much consequence except near the banks, because elsewhere the velocity of that part of the stream into which the float deviates will differ very slightly from the velocity of that part which it leaves. The resultant effect of all the faults is a distortion of the velocity-curves obtained.* Fig. 1 represents

* 'Roorkee Hydraulic Experiments,' chap. x., art. 10 and 11.

the case in which the maximum velocity is at the surface. The upper float and connector always accelerate the lower float, and the lift brings it into a part of the stream where the velocity exceeds that at the assumed depth. Hence the velocity obtained is always too great, and the "observation curve" which is shown in dotted lines lies outside the true curve. When the maximum velocity is below the surface, the curve is distorted, as in Fig. 2. These figures show the general forms of the curves and the average effects of the distortion. The individual curves obtained in the experiments were more or less irregular.

In the twin pattern of double float, the submerged part of the upper float is made of the same size and shape as the lower float, and the velocity of the instrument is assumed to be a mean between the current velocities at the surface and at the level of the lower float. The surface velocity is observed separately and eliminated. There does not appear to be much to choose between the twin pattern and the ordinary pattern. The forms of the velocity-curves show that the current-action on the connector must generally be less with the twin pattern; but as there are in this case two large bodies, both moving with a velocity which is different from those of the portions of the current in which they are respectively situated, there must be a greater inclination of the connector than in the case of the ordinary pattern, and also more disturbance of the water. The twin pattern is also in some cases more troublesome, because of the necessity for making separate observations of the surface velocity. This is the principal reason given* for its rejection in the Roorkee experiments, but as in this case the surface velocity had to be measured in order to obtain the velocity curves, it is not clear how the objection holds good.

The question now arises whether the floats used were such as were likely to give good results. In order to determine this it is necessary to consider how a double float of the ordinary pattern should be designed. In order that the lower float may be as free as possible from instability, its shape should be such as to afford very little hold to vertically-moving masses of water. At the same time it should afford a good hold to the horizontally-moving current, in order that its velocity may be little affected by the current-action on the upper float and connector. Obviously, therefore, the best form of lower float is one consisting entirely of vertical plates, say of two vertical plates cutting each other at right angles, with smooth surfaces, and upper and lower edges sharpened. The experimenter states†

* 'Roorkee Hydraulic Experiments,' chap. ix., art. 26.

† Ibid., chap. ix., art. 8–viii.

that a float of this shape is liable to tilt, and so expose a greatly reduced area to the current, and tilting would also cause it to lose its freedom from instability; but this fault can be corrected by attaching the connector at the centre of the lower float (where the resultant horizontal current-pressure will, on the average, act), and by ballasting it, making the upper and lower parts respectively of very light and very heavy materials, say wood and lead. Assuming the thickness of the plates to be uniform, the resistance to tilting will be a maximum if the heights of the heavy and light portions are inversely as the square roots of the specific gravities of the materials. A float made of two vertical tin plates was tried by Major Cunningham, and found to be very liable both to tilting and to instability, as was to be expected. The connector should be of wire, or such material as admits of the greatest degree of thinness and smoothness. The upper float should be of light material, say hollow metal, so that its submerged volume may be a minimum. It should be very smooth and spindle-shaped, so as to offer a minimum resistance both to air and current, the connector being attached rather towards one end, so as to make the float point in the direction of the resistance. Hollow tin ellipsoids were used in some of the American experiments.

Next as to the best dimensions of the instrument. The force tending to cause instability of the submerged float depends chiefly on its superficial area, and the force resisting instability is its weight. Its stability depends therefore on the ratio of its weight to its superficial area, that is, on the thickness of the plates independently of their length and height. For all floats of the same shape and material there will be a certain thickness of plate which will be the least consistent with stability, and a float must always be composed of plates of this minimum thickness, in order that the thickness of the connector and submerged volume of upper float may be as small as possible. Of any two double floats designed in this way, that which has the larger submerged float will be the more efficient. The current-action on any part of the instrument may be taken to be proportional to its direct area, that is, its area as projected on a plane perpendicular to the direction of the stream. If the direct areas of the lower floats are as 4 and 1, their weights are as 4 and 1, and the submerged volumes of the upper floats are as 4 and 1. But the direct areas of the upper floats, supposing their shapes to be similar, will be as $\sqrt[3]{4}$ and 1 or nearly as 2·5 and 1. Therefore in the larger instrument the current-action on the upper float is relatively much smaller. The thicknesses and direct areas of the connectors are also theoretically as 2 and 1, so that here again the larger instrument has greatly the

advantage, and practically if the lower float be small, it is physically impossible to make the connector thin enough. The larger float will also probably tilt less, because of the greater tendency of the irregular current-actions on different parts of it to neutralise one another, and for the same reason its velocity will be less variable. This reasoning, except as regards the current-action on the upper float, applies to the twin pattern as well as to the ordinary pattern ; and, except as regards tilting, it holds good even if the lower float be of any other shape than that assumed, for instance, a hollow ball ; and if the current-action be taken to be as the superficial area, instead of the direct area. The conclusion, therefore, is that the lower float should always be of the largest size of which the circumstances admit.

Major Cunningham says that the lower float should be small, so that it will not interfere much with the natural motion of the water,* but it will not do this unless it is very large. The size is limited by the consideration that the velocities at all the points included within the area of the float should not differ very much among themselves, and that their mean should be sensibly equal to the velocity at the centre of the float. The height of the float may be, say, one-tenth of the whole depth of water, except for observations near the line of maximum velocity, that is, near the surface, where it should be less ; and the width may be the greatest consistent with freedom from tilting, except for observations very near the sides of the channel. The deeper the stream, the greater the admissible size of float. It is frequently stated that, owing to the greater length of the connector, the efficiency of the instrument decreases with the depth of the lower float below the surface, and that double floats are quite unfit for use in deep rivers. This idea appears to be erroneous. It is quite true that the efficiency of the instrument decreases if the same float be used at different depths, and in any stream it will be possible to obtain more reliable observations near the surface than near the bed, but if the size of the float be proportioned to the size of the stream, the results obtained will be as good in a very deep river as in a very shallow one.

The floats used at Roorkee consisted of wood or metal balls, 3 inches or $1\frac{5}{8}$ inch in diameter, connected by thread or wire to small pine or cork discs. When the lower float was 10 feet below the surface the area of the connector exposed to direct current-pressure was, with the smaller instrument, ·48 of that of the lower float. It is evident that the results obtained under these circumstances cannot have been good. The floats were unsuitable both in shape and material, but the great fault was the smallness of the lower float. The depth of water at the

* 'Roorkee Hydraulic Experiments,' chap. iv., art. 6.

sites of the experiments was usually from 6 to 10 feet. If the floats had been, say, from 6 inches to a foot high and of proportionate width, the velocities would have been measured with far greater accuracy. As it is, when the lower float was near the bed, the connector effect, and probably the instability, were so great that the results obtained can have been only approximations to the real velocities.

It is also a question whether the velocities could not have been obtained better by the current-meter than by double floats, however well designed. The experimenter considered the current-meter to be a very untrustworthy instrument, and he mentions a number of faults which he believed it to possess.* Most of these faults are trivial, and most of them vanish when the constants of the instrument are properly determined and when it is properly constructed and used, especially if the recording parts are above water. The double float is best suited to a slow current; for, the greater the velocity of the stream, the greater will be the differences among the velocities at different levels, and the greater the current-actions on the instrument and the lift of the lower float. The greater also the strength of the eddies and the instability. The double-float is better than the current-meter only if the velocity is very low, or if the stream contain much floating weed. At Roorkee the velocity was often 4 or 4·5 feet per second. The current-meter would probably have given better results than any double float, and it would certainly have effected a very great saving of time.

Rods.—These instruments were used in connection with the double floats, the object being to ascertain whether a rod reaching nearly to the bed gave a good measurement of the mean velocity from the surface to the bed, or, as it may be termed, the "mean velocity on the vertical." The experimenter makes a long mathematical investigation into the theory of rod motion and proves that the length of a rod, in order that it may give the mean velocity on the vertical, must be about ·94 of the full depth of water.† He considers this result important, because it has often been supposed that rods not reaching to the bed, and therefore not dipping into the slowest part of the stream, give velocities which are too great. In the investigation he assumes the average velocity curve to be a parabola, but it is now generally believed that the curve is of quite a different nature close to the bed, the velocity decreasing very rapidly as the bed is approached, as shown by lines dotted

* 'Roorkee Hydraulic Experiments,' chap. xxiii., art. 3.
† Ibid., chap. xvi.

thus - — - — - —, in Figs. 1 and 2. A rod reaching to ·94 of the full depth will therefore move too quickly. It ought probably to reach close to the bed. From the manner in which the velocity curves are distorted, owing to the double float error (Figs. 1 and 2), Major Cunningham shows that the mean velocity on the vertical, as obtained from the double float observations, is too great when the maximum velocity is near the surface, and too small when it is very deeply submerged, say at mid-depth.* The rod velocity was found in almost every case to exceed or fall short of the mean double float velocity, according as the latter was known to be too small or too great; and looking at the amount of the difference it can be seen that the rods gave better results than the double floats. This, however, does not amount to a proof of the correctness of the rod velocities and, as the length of rod did not on the average exceed ·90 of the full depth, there can be little doubt that the rod velocities are somewhat too high.

The experimenter considers that, in cases favourable to its use, the rod should supersede all other instruments for the measurement of the mean velocity on a vertical.† There can, however, be no doubt that the current-meter is in the great majority of cases the best instrument. It can be used in any stream whether regular or not, and the mean velocity on the vertical can be obtained by letting it descend from the surface to the bed and drawing it up again. The result is also obtained much more rapidly than with floats. Of course there are cases in which floats are best, or where current-meters are not available, and in such cases there does not seem to be much to choose between rods and double floats. The mean velocity can be obtained pretty accurately by observing the velocity at one point with a double float, namely at about ·60 of the full depth, or better still by observing it at two points. Major Cunningham has shown that these two points should preferably be at ·211 and ·789 of the full depth, and that the mean velocity can be measured by a special float consisting of a small surface float and two large submerged floats moving respectively at these depths.‡ The rod is, however, the more convenient instrument unless the depth be great, and, owing to its measuring the mean of the velocities at a great number of points, the unsteady motion of the water is to some extent eliminated, and a good result can be obtained with fewer repetitions of the observations than would be the case with a double float.

* 'Roorkee Hydraulic Experiments,' chap. xiii., art. 4.
† Ibid., chap. xv., art. 10.
‡ Min. Proc. Inst. C.E., vol. lxxi., p. 19.

II.—Velocity Curves.

Vertical Velocity Curves.—The observation of the velocity three times in succession at each foot of depth on a given vertical line in the cross section of the stream constituted what was called a "set." The vertical was generally in the centre of the stream. The velocity curves were obtained in each case by taking the mean values of several sets which had been observed on the same vertical, and when the water was at nearly the same level; but, owing to the unsteady motion of the water, the curves are all more or less irregular. The general forms of the curves are shown in Figs. 1 and 2. The maximum velocity line is sometimes at the surface but generally below it, and in the rectangular aqueduct channel it sinks towards the sides of the channel, where it is at about mid-depth.

In order to obtain regular curves and investigate their properties, the experimenter applied to each irregular curve that parabola, with horizontal axis, which was found by the "method of least squares" to fit it best. In order to allow for the double float error, which is known to be greater the further the lower float is from the surface, less "weight" was given to the observations the nearer the bed; so that the parabola generally fits the curve less closely near the bed. This, however, does not seem to have been the proper method of procedure, inasmuch as it takes no account of the probable kind of the errors. The process adopted would be suitable, if it were known simply that the probable error was greatest near the bed, without anything being known as to the probable direction in which the error lay. Major Cunningham has, however, shown, as explained above, that the average error is of a particular kind at particular parts of the curve, and it is obvious that this should have been considered. Near the bed the observed velocity is always too great, but when corrected by the process adopted it comes out sometimes as great as or even greater than before. The proper method would seem to be to calculate the amount of error in each case, apply corrections of the proper kind, and then find the regular curve which most nearly fits the corrected velocities.

There is really no reason for supposing that the velocity curves are parabolas. Except in an indefinitely wide stream with a level bed the curves on neighbouring verticals are not alike, and the velocities must influence each other in such a manner as to render the curves probably of a highly complex nature. The curves are, however, except near the bed, so flat

(or rather the differences among the velocities are so small compared to the velocities themselves) that "almost any geometric curve can be fitted pretty closely to them" and the upper part of the curve may therefore without appreciable error be taken to be a parabola.

The "probable error" of the parameter of the computed parabola was often very large, and the laws which determine the length of the parameter and the depth of the axis below the surface cannot be discovered. The tables, however, contain abundant evidence that the value of the parameter on the central vertical increases rapidly with the depth of water on the vertical,* or in other words, the curvature of the curve is sharper the less the depth on the vertical. The form of the curve being unknown, the ratio of the mean velocity on the vertical to the surface velocity is also unknown. The ratio of the rod velocity to the surface velocity (on the central vertical) varied in its mean values from ·87 to ·96 at different sites and under different circumstances, and it appears, therefore, to be impossible to infer the mean velocity on a vertical with much accuracy from observations at the surface alone, although it can be obtained approximately by observations at one or two points below the surface as mentioned above.

The experimenter comes to the conclusion that the depression of the maximum velocity below the surface is due largely to the resistance of the air.† The author is of opinion that the air resistance is insufficient to produce more than a very minute depression. Moreover, the air resistance does not account, as the experimenter says it does, for the increase of the depression towards the sides in a rectangular channel.‡ In a closed rectangular channel, in which the resistance of the top is as great as that of the bed, the maximum velocity is at mid-depth on all verticals. It does not sink towards the sides. In an open rectangular channel the resistance of the air is less than that of the bed, and the maximum velocity line, supposing its position to be affected by the air, should be above mid-depth, but at a constant depth on all verticals.

Transverse Velocity Curves.—In order to obtain a discharge measurement, the velocities on from 10 to 20 verticals in the cross section of the stream were observed by means of rods. The velocity on each vertical was observed three times in succession. This was sufficient to give a good value of the discharge, but in order to obtain a good value of the velocity

* 'Roorkee Hydraulic Experiments,' Table 3.
† Ibid., chap. xii., art. 7.
‡ Ibid., chap. xii., art. 6.

on each vertical separately it was necessary to combine several discharge measurements, all made when the water-level was nearly the same, and in this way the "transverse mean velocity curves" were obtained. Specimens of the curves and cross sections of the channels at the sites where most of the work was done are shown in Plate I.

The experimenter mentions a number of properties which he found these curves to possess, and in defining these properties he speaks of the "flatness" of the curves.* If this expression be used in its ordinary sense, namely, ratio of length of chord to maximum ordinate drawn from the chord to the curve (like the ratio of span to rise in an arch), then many of these properties reduce themselves to well-known truths, such as the following, namely: "at any given site the velocity on any vertical depends both on the depth of water in the channel and on the surface slope, and is constant if both these are constant: and in a wide stream the ratio of the maximum velocity to the surface width is generally less than in a narrow stream." There are, however, two properties which appear to be of importance, namely:—

(1) That if, at any given site, the mean velocity of the stream is changed while the water-level remains constant, the velocities on all verticals are changed in nearly the same proportions, or the transverse curves are all parallel projections of one another.

(2) That if, at any given site, the water-level is changed while the mean velocity of the stream remains constant, the curves are all "equally flat."

The fact that the mean velocity did not always increase with the depth of water was owing to the "control," or ponding up of the stream at the regulating works below the sites of the experiments. Before discussing these two properties it is necessary to notice certain peculiarities in the velocity-curves obtained in the aqueducts.

The left aqueduct was closed for repairs by clay dams at both ends in August 1876, and again in October 1877. While it was closed the whole of the supply was forced through the right aqueduct, and, owing to the obliquity with which the water entered it, the velocity-curve was distorted (see Plate I.). The dams were not thoroughly removed until August 1878, when the canal was laid dry. The left aqueduct was, therefore, partly obstructed for a period of two years, and, when the depth of water was less than 4 feet, the supply entering the left aqueduct was considerably less than that entering the

* 'Roorkee Hydraulic Experiments,' chap. xvii., art. 12.

right, and there was again a distortion of the velocity-curve in the right aqueduct, somewhat similar to that which occurred when the left aqueduct was closed. The distortion was greater the lower the water, and at high water it vanished, the curves in the two aqueducts being almost exactly alike. But at high water there was another kind of distortion, the velocity on any vertical in either aqueduct on the side nearest the central wall being somewhat greater than that on the corresponding vertical on the side next the outer wall (see Table II., p. 56). This was no doubt due to the mid-stream position of the maximum velocity in the channel just above the aqueduct.

The first of the two properties to be considered is that if, at any given site, the mean velocity of the stream is changed while the water-level remains constant, the velocities on all verticals are changed in nearly the same proportion, or the curves are parallel projections of one another. The existence of this property, or rather law, seems probable on general grounds. The relation of the different velocities to one another depends on the form of the channel. The velocity is least near the bed and sides and is greatest where the depth is greatest. If the general velocity of a stream be changed while the depth of water, and therefore the shape of the cross section, remains unchanged, it seems very probable that the mean velocity on every vertical, if not the velocity at every individual point, will change in the same proportion. The existence of this property was inferred by the experimenter from a consideration of five pairs of series, but this is not all the evidence available. To the five pairs of series the author has added five more, and the results of the comparisons are shown in the accompanying Table I.*

The velocity on each vertical in the first series of a pair (the series of higher velocity) is divided by a number representing the ratio of the mean velocity of that series to the mean velocity of the second series, and the result, which is called the "reduced velocity," is compared with the mean velocity on the corresponding vertical of the second series. The two quantities would in every case be equal if the observed velocities were exactly proportionals, but there is generally some difference. The last two columns of the table show the average amount of this difference. It will be seen that the greater the number of sets composing each series, and the more reliable therefore the velocities obtained, the less the difference generally is. At the embankment site the mean velocity of the first series was on the average more than double that of the second series, and

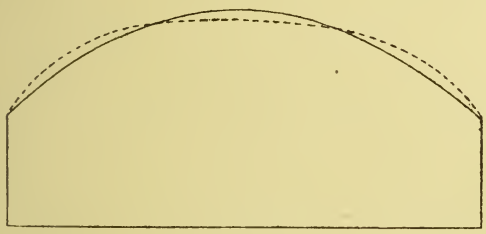
* The pairs of series used by the Experimenter are marked thus: *

TABLE I.

Site.	Reference number of pair of series.	Reference numbers of series.		Gauge Reading		Number of Sets		Ratio of mean velocity of first series to mean velocity of second series.	Mean velocities of stream.		Reduced mean velocity of first series.	Average difference between velocity on a vertical of second series and reduced velocity of first series on corresponding vertical.	
		First series.	Second series.	Of first series.	Of second series.	In first series.	In second series.		First series.	Second series.		Actual.	Per cent. of mean velocity of second series.
Right Aqueduct. Left Aqueduct Closed.	1	135	137	3·18	3·13	1	1	1·29	3·20	2·51	2·51	·38	15·2
	2*	134	133	3·58	3·60	1	1	4·67	3·22	·69	·69	·076	11·0
Right Aqueduct. Left Aqueduct Open.	3*	124	123	3·49	3·65	1	1	3·42	2·43	·71	·71	·050	7·0
	4	125	126	2·02	1·92	1	1	1·30	1·61	1·24	1·24	·063	5·10
Embankment at low water.	5*	170	172	4·82	4·76	2	2	2·27	1·50	·66	·66	·04	6·0
	6*	177	178	3·60	3·61	4	2	1·85	1·50	·81	·81	·03	3·8
	7*	170	171	4·82	4·80	2	3	1·74	1·50	·86	·86	·02	2·5
	8	170	171 / 172	4·82	4·78	2	5	1·92	1·50	·78	·78	·025	3·2
	9	167	171 / 172	5·19	4·78	10	5	2·74	2·14	·78	·78	·024	3·6
	10	169	171 / 172	4·92	4·78	10	5	2 42	1·89	·78	·78	·025	3·1

the differences are only 3 or 4 per cent., which are not greater than must be expected in work of this kind, particularly as slight changes took place in the bed during the period over which the observations extended. The series comprising pairs 1 and 2 each consisted of only one set, and they were observed when the left aqueduct was closed and the flow rendered irregular. If there were any general tendency on the part of the velocities of one series to exceed the reduced velocities of the other series at particular parts of the cross section, and fall short of them at other parts, in the manner indicated in Fig. 3, this would be evidence that the velocities were not proportionals; but there is no such tendency. The curves cross each other irregularly.

Fig. 3.



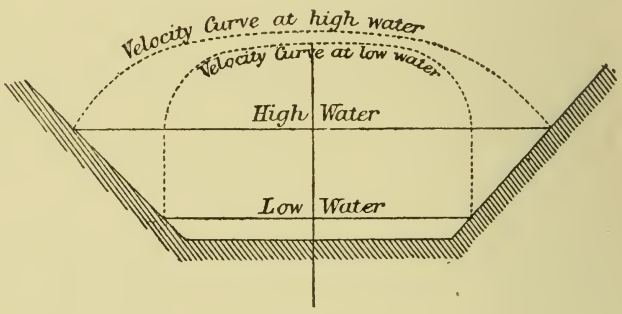
If the velocities are always proportionals in a given channel for a given water-level, they will of course be proportionals in any two equal and similar channels of equal degrees of roughness, in which the depths of water are equal; and probably very nearly proportionals even if the degrees of roughness are not equal, since the effect of an increase of roughness will be the same as that of an increase in velocity, namely increase of the friction at every point of the border. Whether or not an analogous property exists in vertical velocity-curves obtained on the same vertical with the same water-level but different velocities, the evidence furnished by the Roorkee experiments is not sufficient to show.

The other property to be considered is that if, at any given site, the water-level is changed while the mean velocity of the stream remains constant, the transverse curves are "equally flat." In attempting to prove the existence of this property, the experimenter merely refers to certain curves, and states that they can be seen to be on the whole of nearly equal flatness.*

* 'Roorkee Hydraulic Experiments,' chap. xvii., art. 12.

Whether the term "flatness" is here used in its strict sense, or with reference to the general shapes of the curves, it is easy to prove that the property under consideration does not exist in channels which are not of rectangular section. In a trapezoidal channel there is a great rounding off of the curve near the banks, owing to the smaller depth of water, * and it is evident that the lower the water-level the less the relative length and area of the rounded portion of the curve will be. Fig. 4 shows the forms of the curves for high and low water, the surface slope being supposed to be greater at low water, so that the mean velocity is the same as at high water. The two curves obviously cannot have the same shapes nor be equally flat. If the depth of water is indefinitely increased the section of the stream becomes triangular. If the depth is indefinitely small the

Fig. 4.



section is that of a stream of indefinite width compared to its depth. And even in a channel having vertical sides the property cannot exist if the bed is not level. It is a well-marked feature of transverse curves that the velocity is greatest where the depth is greatest. At the embankment site at low water the shallow in the middle causes the central velocity to fall considerably short of those at the points of greatest depth, while at high water the effect of the shallow is imperceptible. At very low water the velocity in the centre would become zero. The velocity-curves at high and low water cannot therefore have the same shapes.

In a rectangular channel, however, there is no variation of depth, the velocity decreases only towards the sides and the decrease is caused by the resistances of the sides. Whatever

* 'Roorkee Hydraulic Experiments,' chap xvii., art. 12.

the depth of water may be, the submerged length of side which produces the resistance always bears the same ratio to the sectional area of the stream whose velocity is affected by the resistance. It is not at all improbable that, for a given mean velocity in the channel, the velocity on any given vertical is always the same, or very nearly so, whatever the depth of water may be, and that the velocity-curves are not only equally flat but are in fact one and the same curve. The Roorkee experiments do not furnish any good direct evidence that this is the case. There are several pairs of series observed in the aqueducts in which the mean velocities are nearly equal and the depths different, and on examining them it is found that in every pair the velocities on corresponding verticals are nearly equal, but the differences in the depths are not great enough to make the evidence of much value. There is however further evidence which goes some way towards proving the existence, in a rectangular channel, of both the properties which have just been considered, although it cannot be adduced to prove each property separately. It is evident that, if both properties exist, the velocities on corresponding verticals in any given rectangular channel are always proportionals, whatever the depth and whatever the velocity. In order to ascertain how far this is the case, all the series at the aqueduct sites, excepting those which give distorted curves (namely, those observed in the right aqueduct when the left aqueduct was closed and four others observed at very low water), have been combined into four groups according to water-level and the mean velocities of the groups on each vertical compared. The combination into groups is necessary in order to get rid of irregularities due to the unsteady motion of the water. The results are shown in the annexed Table II., p. 56.

The velocities on the central vertical and on the four verticals nearest to it are practically equal, and their mean value has in every case been assumed to be 100. The velocities on the other verticals have been altered in proportion. If the velocities are proportionals the values given on any one vertical should all be equal. On examining the table for each aqueduct separately it will be seen that they are on the whole very nearly equal, and as the range of depth (from 10·0 to 3·7 feet) is considerable, the evidence is good. In the left aqueduct the velocities are greater than in the right aqueduct, giving flatter curves. In the left aqueduct the velocities of the low-water group are greater than those of the high-water group, but this is not the case with the corresponding groups in the right aqueduct. In the right aqueduct groups 1 and 3 are almost identical, while in groups 2 and 4 the velocities are less

TABLE II.

Site.	Group.	Depth of Water.	No. of sets in Group.	No. of sets in which left aqueduct was obstructed.	Mean velocities on Verticals.												
					Verticals on side next dividing wall.							Central Vertical and four adjoining Verticals.	Verticals on side next outside of channel.				
					Distance from centre of channel.								Distance from centre of channel.				
					$41\frac{7}{8}$	$41\frac{1}{4}$	40	37·5	35	32·5	30		30	32·5	35	37·5	39·5
Left Aqueduct	1	10·0 to 8·9	31	3	80	85	93	96	98	100	99	100	95	94	92	89	87
	2	8·7 to 7·4	14	14	82	86	94	96	99	99	99	100	99	98	95	92	88
Right Aqueduct.	1	10·0 to 8·9	73	34	79	83	91	96	99	100	99	100	97	94	91	87	86
	2	8·7 to 7·4	47	28	78	82	90	96	98	99	98	100	97	96	93	89	85
	3	7·1 to 5·6	36	27	80	82	91	97	99	99	99	100	96	94	91	87	82
	4	4·5 to 3·7	14	0	77	82	89	94	97	98	98	100	96	95	93	89	80

than in groups 1 and 3 on one side of the centre and greater on the other. There is nothing to indicate change of any particular kind with change of water-level. Such discrepancies as do occur are probably to a great extent due to the varying proportions of the numbers of sets observed, when the left aqueduct was obstructed by the remains of the earthen dams, and to the variable influence exerted at different water-levels by this obstruction, and by other peculiarities of the channel just above the aqueduct.

III.—Discharges and Coefficients.

The method adopted for measuring the discharges has already been stated. The accuracy of the measurements was in many cases tested by the comparison of the results obtained at different sites. The tests were best when, the canal being in train, the gaugings were taken simultaneously at two sites, or when those at the lower site were taken after those at the upper site, sufficient time being allowed for the water to pass from the upper to the lower site. In these cases the discrepancy between the two results was nearly always under 3 per cent., which is certainly small. The tests included several cases in which the result of a gauging taken in the main canal was compared with the sum of the results obtained in the two large branches into which it divides. It is clear that the system of discharge measurement adopted gave results which were consistent one with another, but, owing to the rods not reaching to the bed, the discharges must on the whole have been slightly over-estimated. The average length of rod at the aqueduct sites was ·922 of the depth. At the other sites, where the bed was rough, the rods were probably shorter.

The observation of the rod velocity, U_o, on the central vertical always formed part of a discharge measurement, and with most of the discharge measurements the central surface velocity, V_o, was also observed, its value being found in each case by 48 observations. In order to ascertain how far the mean velocity in the stream can be inferred from either of the above velocities, the experimenter found the values of the coefficients $\alpha = \frac{V}{U_o}$ and $\beta = \frac{V}{V_o}$. The value of either coefficient obtained from a single set of observations is liable to differ by at least 10 per cent. from the value obtained in another set made at the same site and under nearly the same conditions. Although V_o was obtained from 48 observations and U_o from only 3, the fluctuations in the values of β are nearly as great as

in those of a. This appears to be due partly to the liability of the surface float velocities, and perhaps also of the actual surface velocities, to be affected by wind, and partly to the fact that the motion of a rod which measures the velocity on a vertical is steadier than that of a float which measures the velocity at only one point. The fluctuations of both coefficients must also be ascribed partly to changes taking place in the bed, whereby the central depth became altered, to occasional shortness of the rods used for observing the central velocity, and in some degree to errors in the value of V obtained from the discharge measurements. Even when combined into series the values of the coefficients fluctuate so much that the general manner in which they vary is not easily discernible. In order as far as possible to get rid of the fluctuations, the author has combined all the series observed at each site into groups according to water-level, and calculated the mean values of the coefficients for each group. The results are shown in columns 7 and 8 of Table III. (pp. 60, 61).

The experimenter states that the coefficient a "increases in general with decrease of hydraulic mean depth," but on examining the table it will be seen that this is the case at only two or three sites. He also attempts to prove from the properties of the transverse curves that the coefficient "should increase with decrease of velocity," but he states that "the experiments confirm this to some extent only," and the reasoning by which he arrives at the conclusion will on examination be seen to be faulty.* The following appear to be the laws of the variation of the coefficient:—

(1) The coefficient is practically independent of the velocity and depends only on the form of the cross section of the stream. This of course follows from the first of the two properties discussed above (pp. 51 to 57). The table contains no evidence on this point because the depth changes with the velocity. The only instances of change of velocity without change of depth are those which have been already adduced to prove the above property.

(2) In a given rectangular channel the coefficient is nearly the same for all depths. This follows from the second property. It will be seen that the coefficient is practically constant in the aqueducts, except in the group of lowest water-level, and when the left aqueduct was closed, in both of which cases the curves were much distorted, as explained above.

(3) In a trapezoidal channel the coefficient increases as the depth of water decreases, because the rounding off of the curve

* 'Roorkee Hydraulic Experiments,' chap. xx., art. 17.

near the sides is relatively greatest at high water (Fig. 4). For the same reason among different trapezoidal channels, all having the same side-slopes and the same depth of water, the coefficient will be greater the wider the channel. The steeper the side-slopes and the greater the width relatively to the depth, the more nearly the coefficient will approximate to that in a rectangular channel of the same mean width, and the less it will vary with change of water-level. On referring to the table it will be seen that at the Distributary sites, where the width of the stream was small, the coefficients are less than at the Fifteenth mile new site. These are the only sites where the sections were nearly trapezoidal. The changes of water-level are not sufficient to bring out the other laws.

(4) In a channel whose section is irregular or wholly concave, the coefficient may either increase or decrease, or remain constant as the water-level changes. When the depth is great the effect of the irregularities or of the central hollow may be slight, and law 2 or 3 may hold good, but at low water the irregularities will affect the coefficient. A shallow in the middle will cause the coefficient to increase at low water and to approach infinity as the shallow becomes dry. This is well exemplified in the case of the Embankment site. At the other sites (besides those already mentioned) the sections were rough trapezoids, but there were shallows near the sides and other irregularities which sufficed to mask the laws above arrived at for trapezoidal sections. The range of depth was also small. If the depth at the Belra site had varied as much as it did at the aqueduct site the value of the coefficient would probably have varied also.

Regarding the variation of the coefficient β the experimenter does not attempt to deduce any laws, nor is it possible to do so until the relation between the surface velocity and mean velocity on a vertical is known. The value of α depends only on the form of the transverse curve, but that of β depends also on the form of the velocity-curve on the central vertical. It will be noticed, however, that any marked change of one coefficient is usually accompanied by a similar change in the other, and column 9 shows that the ratio of β to α (which is the same as the ratio of the mean velocity on the central vertical to the surface velocity on that vertical) is tolerably constant and never differs much from ·90 except in two of the distributaries. The values of β found in these experiments were compared with Bazin's coefficients and the agreement was found to be very bad. Major Cunningham points out, that Bazin's coefficient is meant to show the proportion of the mean velocity to the maximum velocity, which is generally below

TABLE III.

SITE.	Mean width of stream in feet.	Hydraulic mean depth in feet. [The mean depths exceed the hydraulic mean depths by 10 to 25 per cent.]		Mean Velocity (feet per second).	
		From.	To.	From.	To.
Left Aqueduct	85	7·9	7·5	4·0	3·6
	85	7·2	6·4	3·7	3·4
Right Aqueduct	85	8·0	7·4	4·0	3·8
	85	7·2	6·4	3·9	3·6
	85	6·1	5·0	3·7	3·2
	85	4·1	3·3	2·9	2·4
	85	3·4	0·7	1·6	0·6
Right Aqueduct (Left Aqueduct closed) ..	85	4·2	3·3	4·8	0·7
	85	3·6	2·5	3·2	2·2
Embankment at high water	153	9·3	8·7	4·0	3·7
		8·4	7·3	3·6	3·2
		7·1	5·4	3·4	2·4
Embankment at low water ..	150	5·0	4·7	2·1	0·7
		4·2	1·7	1·8	·04
Fifteenth mile, old site ..	160	9·5	8·6	4·2	4·0
		8·4		3·9	
		8·0	7·1	3·9	3·5
Fifteenth mile, new site ..	170	8·7	8·4	4·1	4·0
Belra	185	9·0	8·7	3·2	3·1
		8·5	8·2	3·1	3·0
		8·0	7·6	3·1	2·9
Jaoli	190	7·8	7·5	3·0	2·9
		7·2	6·8	2·9	2·8
		6·5	6·3	2·7	2·6
Kamhera	60	4·8	4·5	2·9	2·8
		4·4		2·8	
		4·2	4·1	2·8	2·7
Distributaries: Right Jaoli	20	2·8		2·5	
		2·0		2·1	
Distributaries: Miranpur	12	1·6		1·6	
		1·2		1·5	
Distributaries: Pimora	10	2·2		1·8	
		1·9		1·8	
Distributaries: Mansurpur	12	2·3		2·1	
		2·1		2·1	
1	2	3	4	5	6

TABLE III.

Mean Values of Coefficients α and β.			Mean Values of Coefficient C.			Numbers of Sets from which Experimental Values of Coefficients were obtained.			REMARKS.
α	β	$\frac{\beta}{\alpha}$	Experimental.	Kutter's Coefficient.	Bazin's Coefficient.	α	β	C	
·96	No observations		101	153	129	31	—	7	Surface-slope observed at one bank only.
·96	·85	·89	92	150	129	14	1	9	
·96	·88	·92	102	153	129	73	30	40	
·95	·88	·93	100	150	129	47	6	14	
·96	·89	·93	102	147	129	36	13	15	
·95	·88	·92	97	141	128	14	6	7	
1·01	·88	·87	73	113	121	4	1	2	
·93	·85	·91	118	141	128	6	4	5	
·90	·84	·93	115	132	126	6	6	5	
·96	·85	·89	86	87	90	28	18	23	
·95	·84	·89	82	85	88	39	17	26	
·95	·85	·90	87	81	84	32	17	21	
1·00	·92	·92	63	79	80	28	6	6	
1·07	·95	·88	60	69	68	17	13	16	
·93	·86	·92	89	87	90		7		
·96	·86	·90	89	86	89		3		
·95	·86	·91	91	85	87		3		
·95	·88	·93	94	95	89		4		Surface-slope observed at both banks.
·98	·85	·87	75	87	89		12		
·98	·87	·89	76	85	89		23		
·98	·87	·88	75	85	87		18		
·97	·87	·90	82	86	87		15		
·95	·86	·91	88	85	86		25		
·93	·86	·92	88	83	85		15		
·95	·85	·89	77	77	79		19		
·97	·86	·89	78	77	78		11		
·96	·87	·90	76	75	76		26		
·94	·85	·90	No observations.				2		
·91	·84	·92					2		
·95	No observations						1		
·94	·83	·89					2		
·87	·81	·94					2		
·88	·84	·95					2		
·89	·85	·96					4		
·90	·84	·94					1		
7	8	9	10	11	12	13	14	15	16

the surface, and greater than the central surface velocity. The use of this coefficient for determining mean velocities from central surface velocities leads therefore to under-estimation of the mean velocity, but it was recommended by the French Academy of Sciences as giving "sufficient accuracy for practical purposes"* and it has been the practice so to use it in India. Bazin's coefficient is always less than β, generally by 9 and occasionally by 20 per cent., and "the under-estimation of mean velocity from the use of Bazin's ratio is so great in case of earthen channels as to render it of little practical use therein." Bazin's coefficient depends only on the hydraulic mean depth and the nature of the material of the channel. Prony's coefficient for small streams depends only on the velocity, varying from ·75 to ·87 as the maximum velocity varies from 1 inch to 10 feet per second. It is clear that both coefficients must be quite unreliable, since β must, like α, depend on the shape of the cross section. The old rule that $V = \cdot 835\ V_0$ must also be unreliable, although it is apparently better suited to channels like those at Roorkee than either Bazin's or Prony's rule.

The experimenter states that if the central rod velocity had been observed 48 times with each discharge-measurement, the variation of α would have been less than that of β, and that "central mean velocity measurement is to be preferred to central surface velocity measurement for use in approximating to mean sectional velocity." This conclusion appears to be quite correct. He accordingly recommends that, when time does not admit of a discharge measurement being made by observations on a number of verticals, the velocity on the central vertical be observed about 50 times by means of rods and multiplied, to give the mean velocity, by a coefficient which he states must be ascertained by previous experiment at the site. Whatever instrument be used for measuring the central velocity, the usefulness of the recommendation depends chiefly on the question how far it is necessary to determine the coefficient at the site. If an accurate discharge measurement is required the coefficient must be determined at the site and, except when the bed is level and the width great, or the section rectangular, it must be determined by means of discharge measurements at two or three different water-levels, and when these measurements have been made the discharges for all water-levels are known (unless there be disturbing influences below the site) without any further observations being made. But if only a rough discharge measurement is required

* 'Roorkee Hydraulic Experiments,' chap. xx., art. 22A.

then the value of the coefficient may be estimated or assumed to be the same as it had been found to be at some other nearly similar site, and the discharge may with advantage be measured in the manner proposed. If the bed is liable to change, the values of the coefficients, although found at the site, will be unreliable, because the central depth may alter relatively to the other depths. The foregoing remarks also apply to cases in which the discharge is inferred from the central surface velocity, as it may be, but probably with less accuracy, when the central mean velocity cannot conveniently be measured.

It has been suggested that the mean velocity in a channel might be obtained without the use of coefficients, by observing the velocity on a vertical, or on two verticals, situated at given distances from the sides, just as the mean velocity on a vertical can be ascertained from observation at one or two points of the depth. It is obvious however that the distances of these verticals from the side will be different in every channel and, unless the section is rectangular, in the same channel at different water-levels. In some of the narrow channels their distance from the side was about one-fourth of the full width, while in the rectangular aqueduct channel the distance is about one-fifteenth of the width. The vertical velocity curves are more independent of the form of the cross section, and do not vary so much in form as the transverse curves.

Determination of Discharge from Slope and Hydraulic mean Depth.—With most of the discharge measurements the surface slope was observed and, V and R being also known, the values of C in the formula $V = C\sqrt{RS}$ were ascertained. In order to determine how far these values are reliable it is necessary to consider the manner in which the slope was found. The water-level was in each case observed 1000 feet above and 1000 feet below the site of the discharge measurement and, "in order to meet the difficulty of the rapid oscillations of the surface and also of possible gentle oscillations of long period," the water-levels at both the points were always observed simultaneously. The water surface at each point was watched for about half-a-minute, and the mean of the highest and lowest levels observed during that time was taken as the true level. This, however, does not appear to have been sufficient to eliminate the effects of oscillations of long period. At all events slope measurements made at the same site, at the same bank, on closely approximate dates, with nearly the same gauge reading, and when there was no "control," frequently differed from one another by 10 per cent. They were, in fact, "accidental values." The slope should have been measured repeatedly like the velocity. The necessity for this is admitted incidentally in the Roorkee

Hydraulic Experiments,* although it is not mentioned in the chapter on surface slope.

Again, the slopes at opposite banks found on the same day and in the manner above described were often very unequal, the difference being occasionally 15 per cent. and averaging 6 or 7 per cent. These differences cannot have been due merely to the unsteadiness of the water surface, but must have been owing to peculiarities in the channel, as a study of the tables shows that the excess was generally at one particular bank.† In these cases the average water surface cannot have been level across as the experimenter concluded that it was, except in a cross wind, from some other observations which he made.‡ The experimenter comes to the conclusion that the slope should be ascertained from measurements at both banks, and that the measurements at opposite banks should be simultaneous, although both these precautions were omitted in the case of rather more than half of the slope observations. Of the necessity for observation at both banks there can be no doubt, but there is no reason why the observations should be simultaneous either at both banks or at both of the slope points on either bank. The average slope will be obtained by a sufficient number of observations at each bank whether simultaneous or not.

The errors arising from the unsteadiness of the water, that is from the insufficiency of the number of observations, must have been largely eliminated by the combination of the discharges into series. The errors arising from the omission of the observations at one bank would not be so eliminated. Supposing the difference between the average slopes at opposite banks at any one site to be 6 per cent. of the mean of the two slopes (which may be taken as the true slope) the average error would be about 3 per cent., and since C varies as $\sqrt{S}$, this would cause an average error of $1\frac{1}{2}$ per cent. in the values of C obtained.

But, besides the error arising from imperfections in the method of slope observation, there is another error, and in some cases a very serious one, arising from the non-agreement of the observed slope with the local slope at the site of the gaugings. The longitudinal sections (Plate II.) show that in most cases the depth of water varied within the "slope-length." At the aqueduct site it was found that the surface slopes, as ascertained from nearly simultaneous observations of the water-

* Chap. xx., art. 23c.
† 'Roorkee Hydraulic Experiments,' Tables l. to lv.
‡ Ibid., chap. v., art. 11.

levels at the ends of the lengths A B and C D respectively, were liable to differ by 40 or 50 per cent.* No similar observations were made at the other sites, but it can easily be proved that in some cases the slope, as obtained from observations of the water-level at the ends of the 2000-feet length, differed greatly from the slope at the site. The variation of the slope is due to the variation in the cross section of the stream. It is well known that the slope is least where the section is greatest. In a stream consisting of alternate pools and rapids the water in the pools is almost level. In a channel like those under consideration, in which the depth only is variable and the width nearly constant, it can be proved that the slope is nearly inversely as the cube of the depth. Although the flow is not uniform, the velocity at any cross section will be given roughly by the formula $V = C\sqrt{RS}$, where C is a constant coefficient having a value suited to the stream in question. Since the width is constant, and large compared to the mean depth (D), R varies nearly as D, and V varies nearly as $\frac{1}{D}$. Therefore, speaking roughly, S varies as $\frac{1}{D^3}$.

At the Belra site the surface slope in the upper part of the slope length must have very greatly exceeded that in the remaining part, and the local slope was largely over-estimated. The value of C, obtained from the formula $C = \frac{V}{\sqrt{RS}}$, was therefore under-estimated. This would also happen at the embankment site at low water (at high water the relative difference of depth would not be great), and the under-estimation would be increased by the ponding up of the water caused by the control at the tail of the reach. At the other sites (except the aqueduct) there would be no very serious difference between the observed and local slopes, and there was scarcely any control. At the Kamhera site, particularly, the difference would be small.

Regarding the aqueduct site, it is difficult to come to any conclusion. The observations above alluded to showed that the average slope in the lengths C A and B D was much less than the slope in the length A B, and that the difference was greater the lower the water. This was to be expected because of the difference in depth. But it is not easy to determine how the slope in the length A B, which was the length used for the

* 'Roorkee Hydraulic Experiments,' Table lxxix.

slope measurements, differed from that in the aqueduct E F. The mean depth in A E and F B exceeded that in the aqueduct, and the slopes in these lengths would, if the channel had been all of one kind, have been less than that in the aqueduct. But the slope which gives a certain velocity in a masonry channel is only about one-third of that which gives the same velocity in an equal and similar earthen channel, and the slope in the aqueduct may therefore have been much less than that in the lengths A E and F B. If this was so, then the local slope was over-estimated, and the values of C under-estimated. When the left aqueduct was closed and the whole supply was forced through the right aqueduct, the slope in the latter would of course be greater and the under-estimation of C would be less.

The experimenter compared the values of the coefficient C, as determined by him, with those of Bazin and Kutter.* Both Bazin and Kutter have different sets of coefficients for different classes of channels. In order to obtain any sort of agreement, the experimenter had to take the coefficients from that set whose values agreed best with the experimental values, quite regardless of the question whether the channel in which the experiments were made was actually of the class to which the coefficients belonged. For instance, the coefficients obtained in the aqueducts are compared with those of Kutter for "damaged rubble" (rugosity coefficient ·020), but those obtained in the right aqueduct when the left aqueduct was closed are compared with those for "new rubble and old brickwork" (rugosity coefficient ·017). The agreement of Bazin's coefficient with the experimental coefficient was sometimes bad. That of Kutter's was better, and the experimenter comes to the conclusion that Kutter's coefficient is one of pretty general applicability, and that in large canals it will give results whose error will probably seldom exceed 7½ per cent., provided the proper rugosity coefficient be previously determined by experiment at the site,† but that if the coefficient be merely taken from tables so close an approximation will be a mere chance.

If this experimental determination were really necessary, then Kutter's coefficient would be almost useless, since formulæ involving R and S are generally used for finding the velocity in channels which have yet to be constructed, or in

* The rugosity coefficients referred to as Kutter's are those given by Jackson in his "Canal and Culvert Tables," by whom the series has been extended and some of the values altered in accordance with his own observations.

† 'Roorkee Hydraulic Experiments,' chap. xx., art. 23c.

which observations cannot conveniently be made. But there is really no proof whatever of the necessity for finding the coefficient experimentally. In columns 10 and 11 of the table on pages 60 and 61 are shown the experimental values of C (obtained by combination into groups in order to get rid of the fluctuations as in the case of the coefficients α and β), and also the values of Kutter's coefficient for the classes of channel to which the experimental channels actually belonged, namely, in the case of the aqueducts, "brickwork in good order" (rugosity coefficient ·013); in the case of the fifteenth mile new site, where the bed had been levelled and dressed "earthwork above average order" (rugosity coefficient ·0225); and in the case of the other sites "earthwork in average order" (rugosity coefficient ·025). It will be seen that the only sites in the earthen channels at which the agreement is really bad are the Belra site and the embankment at low water, and these are the very instances in which it has been shown that C was under-estimated. At the Kamhera site, where the sources of error were least and the slope was observed at both banks, the agreement is quite remarkable. At the aqueduct sites there is an enormous difference between C and Kutter's coefficient. It is difficult to say whether C was under-estimated or not, but the author prefers to believe that this was the case rather than that Kutter's coefficient is wrong. Altogether the evidence is in favour of the accuracy of Kutter's coefficient, and no necessity whatever has been shown for ascertaining it experimentally at each site. Bazin's coefficient for brick channels in the case of the aqueducts, and for earthen channels in the cases of the other sites, is shown in column 12 of the table. The coefficients for earthen channels agree pretty closely with Kutter's coefficients for earthwork in average order. It must be noticed that at the embankment site the sides were of masonry, and at the Belra and Jaoli sites of bricks laid in mud, so that they were not strictly earthen channels. The length of side was however small compared to the width of the bed which was generally pretty rough.

The formula $V = C\sqrt{R\,S}$ really applies only when the flow is uniform, that is, when the section of the stream is constant. In the channels in which the Roorkee experiments were made the flow was in nearly every case variable within the slope length, but it was probably very nearly uniform within the short length in which the velocities were observed. The formula would therefore apply if the true local slope were observed. The point in which the observations fail is the non-agreement in many cases of the local and

observed slopes. That this was so must be regarded as a great misfortune. Here is a vast series of experiments in which everything is properly observed except the slope. If that had been accurately ascertained the information obtained as to the applicability of Kutter's or other coefficients would have been most valuable. The experimenter states that the Ganges Canal, with its long straight reaches, is extremely favourable for experiment, but he appears to have overlooked the fact that where slope measurements are involved uniformity of section is the primary desideratum. The variation in depth was also increased at the embankment and aqueduct sites by the ponding up caused by the regulation of the supply. It seems probable that much of the disagreement of the results of different experiments as regards the proper values of the coefficient C arises from the want of uniformity in the channels used.

The experimenter says that the connection between mean velocity and other velocities is far more intimate and less complex than between velocity and surface slopes.* There is really no proof that this is the case, although it is of course much better to obtain the velocity by direct observation, on account of the smallness of the slope and the difficulty of measuring it accurately.

The local slope might be observed by constructing two still-water channels, opening from the stream at short distances above and below the site, and both leading into wells separated by a partition, so that the difference of level in them could be observed at frequent intervals of time. The local slope will, however, be accurately obtained by observations at points a great distance apart, if the flow is uniform throughout the slope-length. This will be the case if the cross sections of the stream are all equal throughout the slope-length. It is not sufficient that the cross sections of the channel be all similar and the bed slope uniform, because a weir, bridge-pier, change of slope or section, or other disturbing cause below the site may cause the flow to be variable even in a uniform channel.

For observations which do not involve surface-slope measurements, such as those from which the velocity-curves were obtained, absolute uniformity of flow is not essential. It is sufficient that there be no sudden changes near the site. The distribution of the velocities in the cross section of a stream depends on the form of the cross section, and is not appreciably influenced by the circumstance that the velocity of the whole stream is slowly changing as it flows on.

* 'Roorkee Hydraulic Experiments,' chap. xx., art. 28.

The Roorkee Hydraulic Experiments form a valuable contribution to hydraulic science, and the account of them contains a large amount of miscellaneous information to which space does not admit of detailed reference being made here. The most useful part of the information furnished by the experiments is, in the opinion of the author, that which relates to the distribution of the velocities in the cross section of a stream, especially as to the independence of the velocity-ratios on the actual velocities, the values of the coefficients α and β and the unreliability of the coefficients commonly used for obtaining the mean velocity from the central surface velocity. The two principal faults were in the slope observation and in the pattern of double float used, but fortunately these faults affect only particular results and not the whole of the experiments.

DISCUSSION.

The PRESIDENT said that they had listened to an exhaustive investigation into the correctness of the important series of conclusions arrived at by Major Cunningham from the experiments which he had carried out, and which had been embodied in a valuable treatise and had also been discussed elsewhere. Whether or not those conclusions were disturbed by the present paper, it would constitute a valuable contribution to professional literature. It was much to be regretted that the author was unavoidably prevented from being present. The President then invited discussion upon the paper, observing that they had the pleasure of the presence of Professor Vernon-Harcourt, from whom they would be glad to hear some observations.

Professor L. F. VERNON-HARCOURT said that he had had the pleasure of hearing the paper which was read by Major Cunningham at the Institution of Civil Engineers in 1882, and he had also read Major Cunningham's book upon the subject. It was, therefore, very interesting to hear the criticisms of another gentleman, who was no doubt well acquainted with the subject, particularly as those criticisms seemed to lead to somewhat different conclusions from those at which Major Cunningham had arrived. As to floats for measuring the velocity of currents, it was well known that there was great difference of opinion. Some persons preferred them to anything else. General Abbott's experiments on the Mississippi were conducted entirely by means of double floats. There was, however, great difficulty in determining the exact position of the floats, and also how far the connecting cord affected the velocity, and how far also the upper float affected the velocity of the sub-

surface float, which was the one that was really supposed to measure the velocity. The author stated that it would be advantageous to have floats of large size. He did not know whether plates had been tried. At first sight it would seem as if vertical plates at right angles to each other would be useful, because they would not be so much affected by the upward current which might occur in the case of eddies. If they could be kept vertical they would be very useful for indicating the forward velocity, which was the thing to be measured. The author mentioned that current-meters were in many ways preferable to double floats. If a current-meter was properly rated it would be a very reliable instrument, especially if it was possible to keep the recording apparatus above the water. The difficulty which he found in practice to exist with a current-meter was that sometimes in sluggish streams it did not revolve at all, but he should imagine that in a canal like the Ganges canal that difficulty did not generally arise. He should have thought that it would have been very valuable to have made a continuous series of observations with current-meters, so as to compare them with the float experiments; but he gathered that it was rather want of time and funds, than any objection to the use of current-meters, that led Major Cunningham to rely solely on floats. The current-meter exhibited on the table (not by the author of the paper) did not seem to be the best form of meter. He considered that the one which M. Révy had used, and which Elliot Brothers made, was a better form. But he had himself used one somewhat of the form of that now exhibited, and to a certain extent it answered fairly well for rough measurements. Generally there was a brake attached to the instrument, so that when the current-meter was let down, it was not allowed to revolve during its descent. The brake could be released when the meter was at the proper position in the stream, and the meter set free to revolve. There would be a difficulty, as the author stated, in the use of the current-meter when weeds were floating in the stream, but he thought that a similar difficulty would exist with double floats, if there were weeds growing in the stream. In this case the difficulty would be greater with a double float than with the current-meter, but an experimenter would naturally try to find a place which was tolerably free from weeds. In many instances it might be necessary to take velocity measurements in unfavourable positions, and they must then be rough. But in most cases, except when it was desired to deduce a formula of discharge, the measurements were not required to be of very great accuracy; and approximate observations with a current-meter, or with floats, enabled the engineer to form a better

opinion of the actual discharge than he could calculate merely from taking a cross section and the surface slope.

Major Cunningham seemed to have taken a great deal of trouble to ascertain the exact curve of velocities. They all knew that an engineer rather liked to get a regular curve, whether it was a parabola or any other. Such a curve was undoubtedly convenient to enable them to detect inevitable errors in observation. But there was no special reason why the curve should be a parabola or of any other particular form, for they could not suppose that there was any regular mathematical law for the curve of the velocity.

Rods would be a very satisfactory means of measuring the flow of a stream in places where the depths were constant; but it would be seen at once that supposing, as in most rivers, there were considerable variations in depth along the centre line and in other parts of the bed of the stream, it would be very difficult to select a rod of suitable length which would truly measure the velocity of the stream, for the rod must be short enough to float over the highest shoals and then in the deeper parts it would not reach near the bottom. The advantage of the rod was that it gave the average result at once for almost the total depth along the line which it traversed. In the experiments at Lowell, in America, where the flow along flumes was measured, rods could be used very well; but he did not think that it would be practicable to use rods in the ordinary cases of measuring the discharge in rivers of variable depth. Where the cross section and the longitudinal section were fairly regular, rods ought to give a better result than double floats, because they got rid of the uncertainty as to the position of the lower float, and also of the influence of the connecting cord and of the upper float.

The effect of the wind, or even still air, upon the surface of the stream seemed to be a point of some importance. There had been various views given at different times why the velocities were generally greater below the surface. He thought that the retarding influence of the air was one cause, and also, possibly, the amount of momentum which was given by the surrounding water in motion in the central parts of the stream. It might naturally be supposed that if they examined a central portion of the water, away from the banks and bed, and not near enough to the surface to be influenced by the wind, they would find the greatest velocity in that position. A suggestion was started by Professor Unwin, when Major Cunningham's paper was discussed at the Institution of Civil Engineers, that the upper surface of the water was checked by the lower water, which was directed upwards with considerably

reduced forward velocity by the irregularities in the bottom; but that did not seem to him (Mr. Vernon-Harcourt) a sufficient reason, because the water which thus came up towards the surface would sooner check the motion of the central particles which it would encounter first, than the motion of the upper particles which would be the last reached. Mr. Stearne, an American engineer, had recently suggested another reason, viz., that the particles of water at the sides, going more slowly than the rest owing to friction, rose gradually to the surface of the water in the line of least resistance, and floated in towards the centre, and thus imparted their diminution in velocity to the upper surface, causing a depression in the position of maximum velocity.

He thought that the author had brought forward an important point with reference to the surface slope. If the slope was not accurate, of course it would be unfair to introduce it into a formula for the purpose of comparing the result of the formula with the measured discharge, and if the bottom was irregular, it would not be fair to assume the slope regular over a long distance. Doubtless in a short distance, it would be very difficult to measure the slope accurately. But that would rather point to the conclusion that, in making the slope measurements, the engineer ought to select a portion of the channel which was regular in form and not too close to any interposing obstacle, such as a weir; and then it would be possible to measure the slope in a long distance, so that it might be fairly inserted in the formula. He thought it was well understood now that Bazin's formula was only satisfactory for small streams, where the flow was very regular, but that it was not applicable in the same way to large streams. As to Kutter's formula, though it was complicated, he believed it had given satisfactory results for large as well as small streams; and he was glad to find that the apparent discrepancy between its results and Major Cunningham's measurements might be attributed to some defects in the calculation of the slope, and to some modifications being required in the coefficients of roughness. It was very necessary to have some formula by which to measure the probable discharge of a canal which was going to be made, or of a river which was going to be enlarged. He thought that the value of such experiments as those commented on in the paper would be to ascertain what was a reliable formula, or to enable one to be constructed.

Mr. W. P. Morison said that the description of the floats would have been elucidated by the addition of some diagrams.

Major Allan Cunningham, R.E., said that it was not often that any large work met with so detailed and carefully studied

a criticism as that now read on the "Roorkee Hydraulic Experiments." Such criticism always tended to the elucidation of truth, the sole aim of an experimental research. The review was mainly adverse, as such criticisms usually were. It was right in the interests of science that imperfection in the observations should be made known, and that wrong conclusions should be negatived. But it must be pointed out that very many of the objections now raised to the work were a recapitulation of objections already detailed in the original, and were therefore nothing new; also that, as to objections made that the experimental procedure was sometimes not that recommended in the published conclusions, it was explained in the original that some of these were the consequence of the experiments themselves, and that in some cases practical difficulties prevented the adoption of those recognised *à priori*. Many of the objections made in the paper could be fully met; others could be shown to be matters of opinion. To take them all in detail would need a reply as long as the paper; the main points only would, therefore, be taken up. But before doing so, it would be well to mention that the experiments were carried out on a grand scale, and lasted about four years: perhaps their most valuable feature was the very wide range of conditions and results obtained, e. g.:—

Sites—Masonry, boulder, rubble, earthen.	
Width (of sites)	193 feet to 13 feet.
Central depths	11 feet to 9 inches.
Surface slopes	480 to 24 per million.
Mean velocities	$6\frac{1}{2}$ feet to $\frac{1}{2}$ foot per second.
Discharges	7400 to 100 cubic feet per second.

Surface Floats.—Objection was taken in the paper to those used as exposing too much surface to wind compared with their immersion. Now they were small discs, of 3 inches diam. in pine, and of 1 inch diam. in cork, not more than $\frac{1}{4}$ inch thick; small tufts of cotton-wool were placed on them loosely to serve as "markers" when the plain discs could not be easily seen. It was not easy to see how less exposure to wind could be contrived, along with visibility from the banks. More immersion might have been given, but minimum immersion is the essence of a surface-float.

Double Floats.—The first chief objection raised against the whole work was to the faulty patterns of double float used. The faults now detailed, however, had been already stated in the Roorkee Report; indeed one of the patterns used was therein admitted to be among the worst types used in grand operations of this kind. In deducing conclusions an attempt

had been made to allow for the imperfection of the observations. The paper recommended that the height of the sub-float should be one-tenth of the depth of water, a size which seemed to him (Major Cunningham) too large for either accuracy or convenience. The bulk and weight of the large stock required for use at one time would have been a great drawback: e. g. a stock of 90 (i. e. 9 of each depth from 1 foot to 10 feet) were laid out together in a small rowing boat in the Roorkee work.

Current-Meters.—The experience of these in the Roorkee work was small, but that little was unfavourable. The real reason for the use of floats was a financial one, the funds having been only provided year by year. Floats could be cheaply and quickly provided locally. Current-meters are very expensive, and good ones could only have been obtained from Europe, a process involving a long delay in the Indian Government routine.

Rod-length.—Exception was taken to the assumption of a parabola as the figure of the vertical velocity-curve, in proving that the proper immersion of a rod to measure mean velocity through the full depth is only ·94 of that depth. But the precise figure of the curve did not matter: all published vertical curves known to him (Major Cunningham) were so flat that the parabola fitted them nearly enough, in which case the fraction ·94 was a fair approximation.

The paper suggested that, for measuring mean velocity past a vertical, there was not much to choose between rods and double floats. This was contrary to all experience. The proper depth of immersion of the sub-float of a double float for this use was unknown *à priori*, and was in fact variable; the fraction ·60 of the depth named in the paper was a poor approximation. The triple float (a suggestion of his own) and the rod were both more accurate for this use, but the rod was certainly much the more convenient and rapid in work at suitable sites.

Vertical Velocity Curves.—Exception was taken to the mode in which the parabola fitted to the observed curves had been computed, in that the known direction of the instrumental errors was not properly included in the corrected curves. The defect he admitted. The proper manner of including this, however, seemed to him to be very doubtful as yet.

Transverse Velocity Curves.—The term "flatness" of a curve much used in the Roorkee work had been misunderstood in the paper. A more precise phrase would be "flatness of curvature throughout nearly the whole breadth," or, more shortly, "general flatness." The evidence of the two properties proposed—

1st. With constant water-level, like transverse curves are parallel projections when the mean velocity changes; 2nd. With constant mean velocity, like transverse curves are equally flat (at a rectangular site), when the water-level changes—was now much increased by the additional working up of the Roorkee data in this Paper. He accepted the restriction of the second property to rectangular sites.

Ratio of Mean Velocity to Mean Velocity on central vertical ($V \div U_0$).—The laws of change of this ratio proposed in the Paper seemed preferable to those in the original.

Discharge Tables.—The suggestion in the Paper that from a few discharge measurements at only two or three different water-levels, the discharge at all water-levels at same site could be inferred, unless there were disturbing influences below, did not seem to him (Major Cunningham) a very useful suggestion. In a canal divided into reaches, even if many miles in length, there was often so much means of control at both head and tail of each reach, that the velocities at any site depended quite as much on the state of the control as on the actual water-level at the site, so that some element measuring the effect of the control must always be included in the observations.

Surface Slope.—The second chief objection made in the Paper to the work as a whole was to the mode of slope measurement; but the faults now detailed were to a great extent already set forth in the Roorkee work. The general objection was that the slope-measures obtained were only single or "accidental" measures of a rapidly varying quantity, and were those of only one bank; and the conclusion was come to that the slopes should have been the means of many measurements taken on both banks, and that in this case simultaneous measurement (about which great pains were taken in the experiments), would have been unnecessary. This conclusion might be accepted as the proper method; but repeated measurement, of course, would have taken a long time: this was a difficulty under a hot Indian sun, and would have involved doubling the observer staff.

The Roorkee work gave some means of estimating the force of the first objection. Table LXXVIII. showed 24 measurements of difference of level of water surface at opposite banks taken every few minutes; the results differed by ·034 of a foot which would have given a change of only ·000,017 for slopes. Again, Table LXXXII. showed 22 pairs of slopes (11 pairs at one site, 11 at another), the constituents of each pair having been taken at two or three hours interval; the utmost divergence of a pair was ·000,041, about one-fifth of the slope itself.

It was further objected that the slope obtained was not the

"local slope," but only the slope in a great length (2000 feet). Now it was simply impossible to measure the really local slope, it could only be approximated to by measuring the surface fall in a reasonable length. Engineering practice admits a length of two miles for river slope-measuring. In these experiments it was shortened to 2000 feet: even in this length, the surface fall was sometimes only about $\frac{1}{2}$ inch, a minute quantity to measure in an oscillating water surface. With all these defects, he (Major Cunningham) claimed that these slope measures were at least as good as those of any equally large work. A suggestion was made by the author that slope might be measured by leading the water from two points, above and below the site, in still water channels, into two tanks separated by a thin partition. This was an excellent suggestion on the score of accuracy. It would of course involve a large and often prohibitive outlay, and in many places (e. g. in aqueducts and embankments) the Canal Department would certainly not allow such works to be constructed in the banks.

Mean Velocity Formula ($V = C \times 100 \sqrt{RS}$).—The author raised objections of several kinds to the laborious research about this important formula.

1st. That formulæ involving slope are applicable only to uniform flow, or to streams of uniform section.

2nd. That the selected sites in the Ganges Canal were unfit for their application.

3rd. That the slope measures were too defective.

He (Major Cunningham) admitted that in strictness, formulæ involving slope were only applicable to uniform flow, or streams of constant section and slope. Unfortunately, except in the rare cases of artificial channels with very hard banks, there were no such channels, even on the small scale. But engineering practice urgently required some such formulæ for streams with irregular bed; and these formulæ were—with all their imperfections—habitually applied to such: so that the improvement of such formulæ for such general application was a most important research. Next, the Ganges Canal was one of the largest in the world. In so great a stream uniformity of section must be rare: but there certainly was at several of the experimental sites an approach to uniformity of section for a great length most rare in so large a stream. But even at the sites in a varying bed the research was fully as important, that being the only ordinary condition.

Bazin's and Kutter's coefficients (C).—Exception was taken in the Paper to the mode of comparing these with the experi-

mental results. Both the formulæ involved a certain "modulus of rugosity" expressing the state of the banks and bed, to which numerical values have been assigned hitherto for only a few different kinds of channel. This assignment depended of course on experiments like the present, and there was no finality in the published numerical values, which would be corrected as time went on. The Roorkee Experiments were on so grand a scale, with so wide a range of all the data, that, unless the measurements were bad, they were certainly a test for the formulæ. And the mode of trial adopted gave the formulæ the best chance, viz., in selecting that rugosity-modulus at each site with which the formulæ gave numerical values of C agreeing pretty closely with the experiments: the moduli so used differed of course somewhat from those apparently proper for such sites according to the accepted scales. And, provided the observations were good, the new values of moduli were certainly improved values.

In the Paper the course taken was to accept the usual values of rugosity-modulus, and compare the results from the formulæ with the experimental. A very condensed table was put forward to show this comparison. The agreement was of course seldom so close as that shown in the Roorkee report (depending on suitable rugosity-moduli); and at the aqueduct sites (more than half the work) there was no sort of agreement. Bazin's formula was however admitted to be fundamentally defective, whilst in Kutter's formula no defect of form was admitted, and the disagreement was ascribed chiefly to the defective slope measurements. Now this Table III. was ill-suited for its purpose; it was far too condensed. Kutter's formula for C contained two variable elements, depth and slope: Bazin's contained only the depth (the omission of slope is its fundamental defect). The range of both these variables was very great at some of the sites in these experiments, and they varied independently. This large range of both was a valuable feature of the work. But in this Table III. the slope did not appear at all, and the arrangement by order of depth led to the conclusion that all results at nearly the same depth were averaged, regardless of the variation of slope. If this were so the important effect of variation of slope was quite lost in the table. It was curious that the table appeared to show distinctly that Bazin's coefficient agreed on the whole closer with the experiments than Kutter's, a conclusion which he (Major Cunningham) could not accept.

Next as to disagreement with Kutter's formula. This formula was an empirical one, and the real evidence both of its algebraic

form and of the numerical quantities involved in it (rugosity-moduli included) was comparison with experiments, including of course slope measures. Defective as the Roorkee slope-measures may have been, he claimed that they were fully as good as those of any equally large collection on such large streams upon which Kutter's formula was originally based, and that the evidence in favour of Kutter's formula has been greatly increased by these very experiments in the particular mode of comparison adopted.

Mr. Joseph Bernays said that many years ago he was engaged in levelling and making calculations for water-wheels and turbines and he then used the stream-meter. There were, however, certain difficulties in its application, such as had been mentioned, arising from weeds floating in the stream. He then suggested a peculiar arrangement which had not, he believed, hitherto been tried.

(The speaker explained, by a sketch on the blackboard, an arrangement which embraced a submerged plane or body against which the running water was to impinge, the resistance offered by the body being indicated above the water by suitable apparatus, electrical or otherwise).

Mr. Arthur Rigg said that he had made a great number of experiments with screw propellers, and he used an appliance to ascertain the pressure of water behind the screw. It was much like a wind vane set in the current to be measured, and there was a finger on the top by which the direction of the current could be observed. By turning the vane at right angles to this observed current, a pressure was obtained that could easily be measured by a spring balance. Probably something of the kind might be arranged for measuring current velocities in rivers. The particular current-meter exhibited on the table did not seem satisfactory in design, and he should not imagine that conclusions drawn from its use could be accurate.

The President, in closing the discussion, said that the proceedings had assumed more or less of a judicial character. They had had an important indictment brought by Mr. Bellasis against some of Major Cunningham's methods and conclusions, and the manner in which Major Cunningham had met it, partly by explanations and partly by admission, conduced greatly to arriving at the truth. It was to be regretted that Mr. Bellasis was not able to be present, because the Members might then have heard more respecting his views under the somewhat modified aspect the subject had assumed.

The author (Mr. E. S. Bellasis) afterwards contributed a written reply on the discussion, which, by permission of the

Council, is here appended, to complete the consideration of the subject:—

Author's reply on the discussion.

It is true that many of the faults mentioned had been previously pointed out by Major Cunningham himself or by others, but it was necessary to re-state them before basing arguments upon them. The bulk of the matter in the Paper is new, and its main object is not to draw attention to defects in the experiments but to show that many of the conclusions to be drawn (and some of the most important ones) are different from those at which the experimenter had arrived. The three most important questions considered are—

1. The laws of the transverse curves.
2. The coefficients α and β.
3. The coefficient C.

On the first two points the conclusions of the author are accepted by Major Cunningham. To the remarks made by Major Cunningham on the third question, and others, replies are given below.

Surface Floats.—Immersion of 1 inch instead of $\frac{1}{8}$th or $\frac{1}{16}$th inch would not have been excessive in water 5 or 10 feet deep.

Double Floats.—The Paper shows that floats of the size suggested would not have been too large for accuracy. Their bulk might make them inconvenient in large numbers, but it is not right to use an inaccurate instrument merely because it is convenient to handle.

Rods.—The author quite agrees that the precise form of the curve does not matter, and that, if flat, it may be assumed to be a parabola. The question turns entirely upon whether it is flat or not. The author, in common with many others, believes that the velocity decreases very rapidly near the bed, just as it does near the sides (Figs. 1 and 2). The experiments do not furnish much evidence on this point, as very few double-float velocities were observed close to the bed. The author also admits that a rod is better than a double float, provided the bed be so level longitudinally that the rod can be made nearly to touch it.

Transverse Velocity Curves.—Taking the word "flatness" to have the meaning now given to it, the "properties" still amount to axioms, except the two specially referred to.

Discharge Tables.—The discharge depends only on the water level, unless there are disturbing causes within a certain distance below the site. This is no doubt the case at very many sites in rivers and canals, for instance, above regulating works or the mouths of tributaries, or the heads of branches

whose discharges are liable to control; but such a site would not be used for discharge measurements, if any other were available.

Surface Slope.—The figures now given by Major Cunningham show that the slope at one bank varied by about $\frac{1}{12}$th of the average bed slope, and those at opposite banks differed by 20 per cent. But the defects in the mode of measurement are not the chief source of error. The great fault was that the slope S′ over the 2000-feet length, however carefully measured, was not in most cases the same as the local slope S. In earthen banks the cost of still water channels would not be great, and it would be well repaid, because time would not be taken up in going to and fro to observe the water-level at distant points. It would certainly be cheaper than doubling the observer staff.

Mean Velocity Formula.—It is nowhere stated in the Paper that the Ganges Canal is unfit for the application of the formula $V = C\sqrt{RS}$. On the contrary, it is stated that the flow was practically uniform in the short length in which the velocities were measured, and that is sufficient. But S in the formula means the local slope. The object was to find $C = \frac{V}{\sqrt{RS}}$, and the error lay in putting S′ for S. C was under or over estimated, according as S′ was greater or less than S. It may have been impracticable to have observed S, and in other experiments S′ may have been observed over 2 miles (which would not necessarily make it differ more from S), but these facts do not affect the question at issue, which is that the values of C obtained were in many cases wrong.

Major Cunningham says that engineering practice urgently requires a formula for streams with irregular bed, and that the improvement of formulæ for such general application is a most important object of research. There are already formulæ enough. Besides the above formula there is another for "variable flow," i.e. for cases where the flow is not uniform even for a short length. Both formulæ give the local velocity in terms of the local slope, and both involve the coefficient C. The only kind of research which can be of any use is the observation of the local velocity and the local slope, so that the proper values of C may be ascertained. The attempt to construct or improve a formula so as to make it give the local velocity in terms of the slope over a great length (in which the slope varies), in other words to make the velocity at one place depend upon the slope at other places is quite hopeless. If S cannot be measured the observations must be made in a uniform stream where S′ = S.

Bazin's and Kutter's coefficients.—Of course the published coefficients have no finality, but the disagreement between them and the experimental coefficients cannot be explained by assuming simply that the experimental channels were rougher or smoother than those to which the published coefficients were meant to apply. The experimental coefficients found for the right aqueduct agree with those of Kutter for "damaged rubble" except when the left aqueduct was closed, and then they agree with those of Kutter for "new rubble and old brickwork." The reason of this is that when the left aqueduct was closed, the slope in the right aqueduct (but not in other parts of the 2000-feet length) was very greatly increased—(in order to double velocity, slope must be quadrupled)—the relation of S′ to S was greatly altered, and consequently the values of C obtained were quite different. It seems rash to state that these slope-measurements are as good as those of any equally large work.

The author has not stated that Bazin's coefficients are unreliable, although Kutter's are well known to be more reliable; neither does he "accept" Kutter's values. He has simply compared both with the experimental coefficients C. For the earthen channels, the coefficients given in Table III. are those of Bazin for "earthwork" and those of Kutter (except at Fifteenth mile new site) for "earthwork in average order." The two differ very slightly from each other, and their agreement with C is good or bad according as S′ agrees or disagrees with S. The only cases of great disagreement (C being much smaller than either) are those in which it has been shown that S′ was greater than S, and C was under-estimated. For the masonry channels Bazin's and Kutter's coefficients do not agree so well with one another, and C is much less than either, except when the left aqueduct was closed when C is greater than before, as explained above. It is impossible to tell whether C was under-estimated when the left aqueduct was open, or over-estimated when it was closed, or both, and it is, therefore, useless to attempt to draw conclusions from the results or to use them as a means of testing Bazin's or Kutter's coefficients.

The condensation of the table is not objectionable; on the contrary, it gets rid of accidental fluctuations. It is true that Kutter's coefficient varies with the slope and that the combinations in the table are made regardless of slope, but at most of the sites the slopes did not vary much in any one group, and moreover the coefficient changes very slightly even with a great change of slope. A change of 100 per cent.

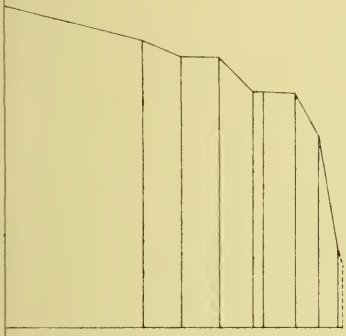
in the slope has less effect on the coefficient than one of 10 per cent. in the depth.

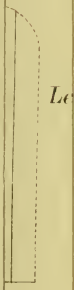
The reason why Bazin's coefficient appeared to Major Cunningham to agree badly with C, as arrived at from his observations, is that when, owing to faulty slope-observations, or other causes, C did not agree with the published values of the coefficients (Bazin's or Kutter's) for the class of channel in question, he compared it with the values for another class. Kutter has far more classes than Bazin, and therefore it was much easier to find one of his, whose value agreed pretty closely with the experimental value, than one of Bazin's.

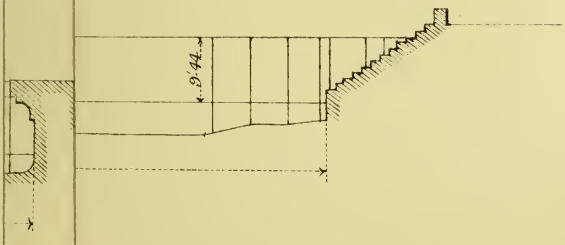
CHA

ss-sec
cities

The dotted portions of the Curves are those where no actual velocities were observed.





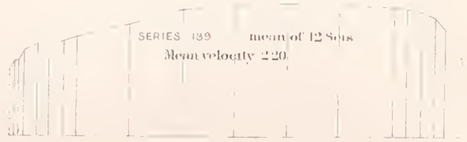


E

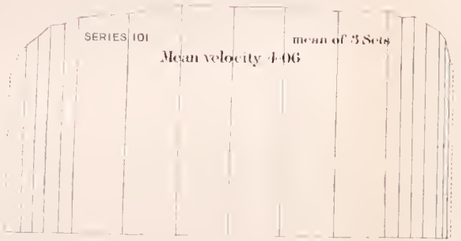
CROSS-SECTIONS OF CHANNELS AND TRANSVERSE VELOCITY CURVES.

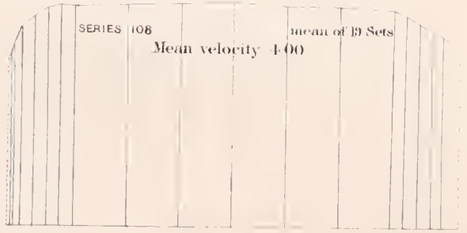
Scale for Cross sections and Abscissæ 25 feet to 1 inch
Scale for Velocities 2·5 feet per second to an inch

The dotted portions of the Curves are those where no actual velocities were observed

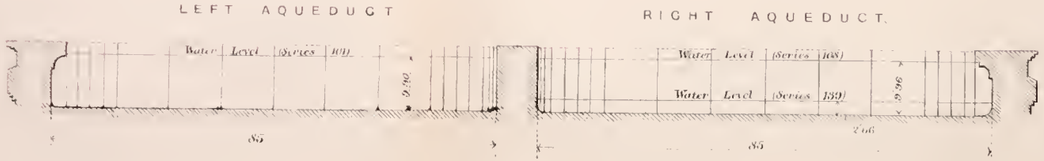


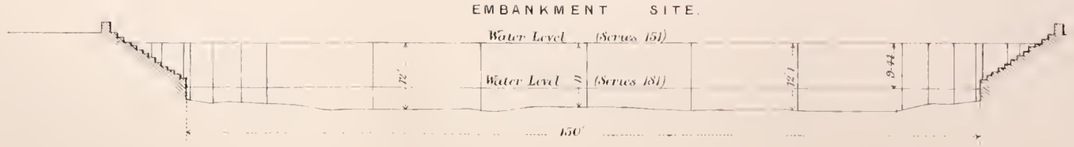
Left Aqueduct Closed





Left Aqueduct Open





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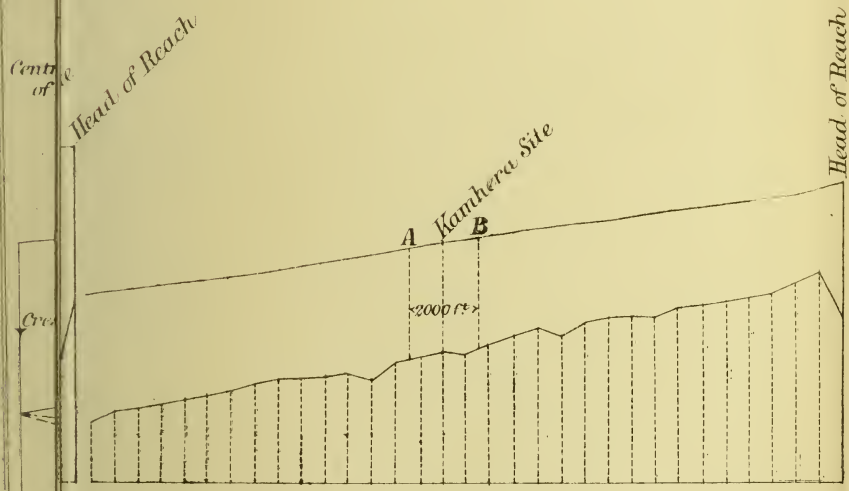

Head of Reach
A
Kamhera Site
B
Head of Reach
2000 ft

LONGITUDINAL SECTIONS OF CHANNELS.

Horizontal Scale, 1 mile to 1 inch
Vertical Scale, 10 feet to 1 inch
At each Site, A B is the Slope length of 2000 feet
At the Aqueduct Site, C D = 4000 feet, and E F = 932 feet

E & F N Spon, London & New York

April 5th, 1886.

PERRY F. NURSEY, President, in the Chair.

OBSCURE INFLUENCES OF RECIPROCATION IN HIGH-SPEED ENGINES.

By Arthur Rigg, Past President.

Few, if any, modern applications of mathematical research have proved so interesting and instructive to mechanical engineers, as those investigations by which Mr. Charles T. Porter first showed how the reciprocation of high-speed pistons might be used to equalise pressures, and conduce to the quiet running of engines driven much faster than the speeds to which all makers of stationary engines had until then been accustomed. His Allen engine, with 12-inch cylinder and 24-inch stroke, first exhibited in London at the Exhibition of 1862, made 200 revolutions per minute and attracted much attention at the time, and the principles then enunciated are still carried out by the Southwark Manufacturing Company of Philadelphia, the makers of these engines.

In most cases of reciprocation, whether in an engine, a printing machine, or other mechanism, it is a crank that rules the rate of acceleration or retardation, and thus the problem involved becomes somewhat unified and converted into a calculation of centrifugal force at the extreme ends of a stroke and of accelerating force at all intermediate positions.

In the term "reciprocating parts" in a steam engine are included the piston, piston rod, motion block with attachments, and all the connecting rod except the head at its outer end. Assuming these to be concentrated around the crank-pin, then its radius in feet ($= r$), the rate of revolution per minute ($= R$) and weight ($= W$) furnish all necessary data for calculating accelerating pressures.

It is convenient, first, to obtain the centrifugal force, as that forms the initial or terminal pressure, and the usual formula employed in this calculation is—

$$\text{Centrifugal force in lb.} = W \times 0{\cdot}000341 \times R^2 \times r.$$

The term $0{\cdot}000341R^2$ is a constant, and may be calculated out for given speeds. This has been done in the preparation of Table I., which can be used to facilitate any calculations wherein centrifugal force forms an element.

TABLE I.—TABLE OF CONSTANTS (REVOLUTIONS^2 × 0·000341).

$50^2 \times C = 0{\cdot}852$	$235^2 \times C = 18{\cdot}832$	$440^2 \times C = 66{\cdot}018$
$55^2 \times C = 1{\cdot}031$	$240^2 \times C = 19{\cdot}642$	$450^2 \times C = 69{\cdot}052$
$60^2 \times C = 1{\cdot}228$	$245^2 \times C = 20{\cdot}468$	$460^2 \times C = 72{\cdot}155$
$65^2 \times C = 1{\cdot}441$	$250^2 \times C = 21{\cdot}312$	$470^2 \times C = 75{\cdot}327$
$70^2 \times C = 1{\cdot}67$	$255^2 \times C = 22{\cdot}173$	$480^2 \times C = 78{\cdot}666$
$75^2 \times C = 1{\cdot}92$	$260^2 \times C = 23{\cdot}052$	$490^2 \times C = 81{\cdot}874$
$80^2 \times C = 2{\cdot}182$	$265^2 \times C = 23{\cdot}947$	$500^2 \times C = 85{\cdot}25$
$85^2 \times C = 2{\cdot}464$	$270^2 \times C = 24{\cdot}859$	$510^2 \times C = 88{\cdot}694$
$90^2 \times C = 2{\cdot}762$	$275^2 \times C = 25{\cdot}688$	$520^2 \times C = 92{\cdot}206$
$95^2 \times C = 3{\cdot}077$	$280^2 \times C = 26{\cdot}734$	$530^2 \times C = 95{\cdot}787$
$100^2 \times C = 3{\cdot}416$	$285^2 \times C = 27{\cdot}698$	$540^2 \times C = 99{\cdot}436$
$105^2 \times C = 3{\cdot}759$	$290^2 \times C = 28{\cdot}678$	$550^2 \times C = 103{\cdot}542$
$110^2 \times C = 4{\cdot}126$	$295^2 \times C = 29{\cdot}975$	$560^2 \times C = 106{\cdot}938$
$115^2 \times C = 4{\cdot}51$	$300^2 \times C = 30{\cdot}69$	$570^2 \times C = 110{\cdot}798$
$120^2 \times C = 4{\cdot}910$	$305^2 \times C = 31{\cdot}721$	$580^2 \times C = 114{\cdot}712$
$125^2 \times C = 5{\cdot}329$	$310^2 \times C = 32{\cdot}77$	$590^2 \times C = 118{\cdot}702$
$130^2 \times C = 5{\cdot}763$	$315^2 \times C = 33{\cdot}835$	$600^2 \times C = 122{\cdot}76$
$135^2 \times C = 6{\cdot}215$	$320^2 \times C = 34{\cdot}718$	$620^2 \times C = 131{\cdot}08$
$140^2 \times C = 6{\cdot}684$	$325^2 \times C = 36{\cdot}018$	$640^2 \times C = 139{\cdot}674$
$145^2 \times C = 7{\cdot}169$	$330^2 \times C = 37{\cdot}134$	$660^2 \times C = 148{\cdot}54$
$150^2 \times C = 7{\cdot}672$	$335^2 \times C = 38{\cdot}268$	$680^2 \times C = 157{\cdot}678$
$155^2 \times C = 8{\cdot}192$	$340^2 \times C = 39{\cdot}419$	$700^2 \times C = 167{\cdot}09$
$160^2 \times C = 8{\cdot}78$	$345^2 \times C = 40{\cdot}587$	$720^2 \times C = 176{\cdot}774$
$165^2 \times C = 9{\cdot}284$	$350^2 \times C = 41{\cdot}772$	$740^2 \times C = 186{\cdot}732$
$170^2 \times C = 9{\cdot}86$	$355^2 \times C = 42{\cdot}864$	$760^2 \times C = 196{\cdot}962$
$175^2 \times C = 10{\cdot}443$	$360^2 \times C = 44{\cdot}193$	$780^2 \times C = 207{\cdot}464$
$180^2 \times C = 11{\cdot}048$	$365^2 \times C = 45{\cdot}429$	$800^2 \times C = 218{\cdot}24$
$185^2 \times C = 11{\cdot}671$	$370^2 \times C = 46{\cdot}682$	$820^2 \times C = 229{\cdot}288$
$190^2 \times C = 12{\cdot}31$	$375^2 \times C = 47{\cdot}593$	$840^2 \times C = 240{\cdot}61$
$195^2 \times C = 12{\cdot}966$	$380^2 \times C = 49{\cdot}24$	$860^2 \times C = 252{\cdot}204$
$200^2 \times C = 13{\cdot}64$	$385^2 \times C = 50{\cdot}544$	$880^2 \times C = 264{\cdot}07$
$205^2 \times C = 14{\cdot}53$	$390^2 \times C = 51{\cdot}866$	$900^2 \times C = 276{\cdot}21$
$210^2 \times C = 15{\cdot}038$	$395^2 \times C = 53{\cdot}204$	$920^2 \times C = 288{\cdot}622$
$215^2 \times C = 15{\cdot}763$	$400^2 \times C = 54{\cdot}56$	$940^2 \times C = 301{\cdot}308$
$220^2 \times C = 16{\cdot}504$	$410^2 \times C = 57{\cdot}322$	$960^2 \times C = 314{\cdot}27$
$225^2 \times C = 17{\cdot}263$	$420^2 \times C = 60{\cdot}152$	$980^2 \times C = 327{\cdot}496$
$230^2 \times C = 18{\cdot}039$	$430^2 \times C = 63{\cdot}051$	$1000^2 \times C = 341{\cdot}000$

Such elementary subjects are fully treated in text-books on dynamics; but in order to secure the use of one system, it will be assumed that all have read the chapters on reciprocating parts in Mr. C. T. Porter's book on the 'Indicator,' or in the author's 'Practical Treatise on the Steam Engine,' pages 258 to 283. Therein will be found a complete investigation of the whole subject, free from purely mathematical symbols, and easily intelligible to working engineers.

So simple and convincing do these theoretical conclusions appear, that one might fairly anticipate their acceptance and general adoption being followed by great changes in the proportions of ordinary steam engines, because makers and users might naturally be supposed to desire the combined advantages of greater power and diminished cost which accompany the use of higher rates of revolution. But it cannot be said that such expectations have been realised, although many attempts have been made to carry these theoretical conclusions into practice. An examination, however, of many so-called "high-speed engines" advertised shows that they really possess a speed little, if anything, beyond the corresponding speeds of common types. Their proportions are ill calculated for expansive working, and if their very moderate increase of speed be exceeded, their energies seem devoted to self-destruction. But they seem scientific to ordinary users of steam power, and, being cheap, such engines are quite good enough for such people!

Dynamical relationships in small steam engines produce such insignificant effects in comparison to their proportions that no difficulties arise except at extremely high speeds; it is only with engines of large size and power that we are soon brought face to face with heavy dynamical strains which would seem to be difficult to understand or remove. Yet it is well known that many really powerful engines of this class are running with smoothness and even with economy, while others are hammering away an uneasy existence with considerable energy; and mere superficial observers accept such evidences as against high speeds, but ignore everything in their favour. This, however, is a very shallow sort of conclusion, and every scientific engineer would disdain to submit to the dictation of any such inertia of stupidity. He would prefer to learn the true causes that may influence these irregular results, rather than palliate existing evils, much less deny their existence altogether. It would almost seem as if the usual calculations of the influences of reciprocation in large and powerful engines must contrive to pass unnoticed some disturbing influence that is so important as to render the existing theory an imperfect exponent of things as they really occur, and the author now proposes to examine a little into the subject, and endeavour to extract some further light as to why one quick-running engine proves a practical failure, while another made on the same lines of construction may be thoroughly successful; remembering that the successful result of a single large high-speed engine is ample proof in their favour and outweighs a thousand failures.

It may be useful first to refer in general outline to the effects

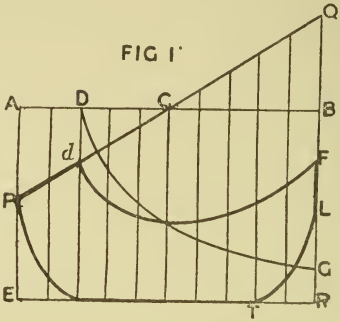
produced upon the driving power of an engine by the necessity for acceleration and retardation of its reciprocating parts, and to remind ourselves of those influences which change the distribution of pressure on a piston into something quite different experienced by the crank-pin. And those who desire to obtain fuller knowledge of the subject should study it thoroughly, for the present paper does not touch upon many points of value. It employs the general construction of diagrams given in the author's treatise already referred to, with which some of the figures are identical, or nearly so. Since 1872 the author has designed and constructed high-speed engines, and has devoted much study to them, and has enjoyed exceptional opportunities for observing their behaviour. This not only in engines of his own design, some of considerable power, but also in engines by other makers. Some of these engines have been far from perfect, while others have been eminently satisfactory from this particular point of view, and people may argue if they please that such results condemn high speeds altogether, or that they prove the general correctness of those theories upon which their construction is based. Few engineers are really aware how many difficulties environ the whole subject; and some users of steam power, more exegeant still, are apt to indulge extravagant expectations. But it may well be remembered that science moves slowly, though resistlessly, and it is unreasonable to discourage attempts to extract its hidden mysteries because of the difficulties encountered, or the partial failures experienced. Such foolish critics should remember that no invention or improvement ever was perfected at once, nor has anybody ever heard of a second Minerva springing fully equipped from the head of Jupiter.

It may be considered unfortunate for the progress of science that so few engineers publish data of an accurate and complete character, particularly in relation to any attempts to improve the scientific bases of their designs, especially when success is reached only through the channel of some comparative miscalculations or even failures. And this reluctance is the more regrettable, because it frequently happens that a failure lends itself far more readily to the acquiring of a real knowledge of truth, than the examination of many a complete success where the stages of progress have been completely lost. This error shall here be avoided and the results be given of actual experience with high-speed engines that are deserving of the name, taking only two types for illustration, wherein the mass of their reciprocating parts has been avowedly used for equalising the incidence of high pressures and early rates of expansion in one cylinder.

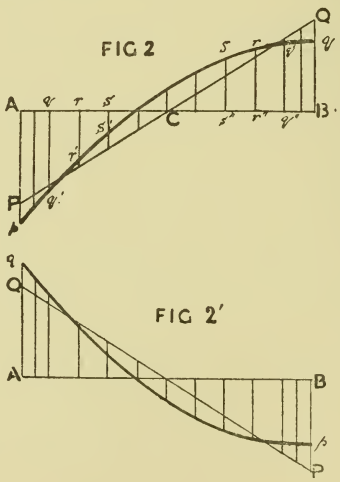
In these two classes full information is at hand, and the author wishes to acknowledge his indebtedness to the Southwark Manufacturing Company of Philadelphia for the valuable information as to the weights of reciprocating parts, which they have so liberally afforded and so freely placed at his disposal. The indicator diagrams from these Porter-Allen engines are taken from published sources, and are intended particularly to show the evils arising from improper arrangements, not for criticism, but only as showing how their proportions may influence the quiet running of engines that make many revolutions per minute. Some diagrams given are thoroughly unsuited to quick-running engines, but they give useful instruction as to what ought to be avoided.

These remarks also apply to diagrams taken from the author's own high-speed steam engines, and it is hoped that this Paper may aid in the removal of many crude fallacies which disfigure engine makers' catalogues and mislead an ignorant public. It should also be explained that all original diagrams have been altered to one uniform scale of $\frac{1}{60}$ inch per lb. pressure, and to a uniform length; and all circles representing the crank path are to a scale of $\frac{3}{4}$ inch to the foot. Irregularities due to oscillations of indicator pencils have been smoothed down, but no radical changes have been made in the original indicator cards. The graphic diagrams used in this paper are similar in general type to those invented by the author, and first published in January and April 1870. They are used in his 'Practical Treatise on the Steam Engine,' and are fully explained there for those who wish to understand them. To show how valuable has been the assistance which engineers have gained from this graphic method of illustrating $\pm$ pressures on a crank-pin, it may be mentioned that it was adopted by the present distinguished president of the Institution of Civil Engineers, Sir F. Bramwell, in his exhaustive paper on marine engines read at the Liverpool meeting of the Institution of Mechanical Engineers in July 1872, and it is now used by engineers generally all over the world. This method replaces very troublesome mathematical calculations, such as few can be expected to remember for long after the completion of their academical studies, much less to undertake in addition to their ordinary labours. An ideal indicator card, A D G R E P, is shown by Fig. 1; 30 lb. of its initial pressure of 60 lb. per square inch is required for overcoming the inertia of its reciprocating parts, transferred as 30 lb. to the further end of a stroke, as a measure of resistance needed to restore these reciprocating parts to a state of rest. Diminishing, accelerating, or retarding pressures are represented by vertical ordinates from A B to the oblique line P Q, and thus the original indi-

cator card becomes changed to Pd F R E, as representing pressures transmitted to the crank-pin, where a connecting rod is of infinite length. A finite connecting rod, of four times the



crank radius, produces the modifications shown in Figs. 2 and 2′ with the proportions crank ÷ connecting rod = 1 ÷ 4 ; the pressure Pp in excess or Qq in deficiency of that due to an infinite connecting rod becomes one-fourth of A P, or 30 lb.—namely, 7·5 lb., which has to be deducted from or added to the initial steam pressure. If the ratio of crank to connecting rod had been as 1 ÷ 6, for example, then no more than 5 lb. would have to be subtracted or added. These considerations explain



why indicator cards ought never to be exactly alike in form for the forward and backward stroke ; and running at a quick speed emphasises the necessity for this difference in order to produce steady driving.

TABLE II.—PROPORTIONS OF RECIPROCATING PARTS IN HIGH-SPEED STEAM ENGINES TAKEN FROM ACTUAL MEASUREMENT.

Maker's Name.	Diameter of Piston.	Area of Piston.	Stroke of Piston.	Revolutions per Minute.	Ratio Crank Connecting Rod.	Total Weights.		Acceleration and Retardation Pressures.				Refer to Figures.	Horse-power.
						Weight.	Per Square Inch of Piston.	Mean.	±	Outward Stroke.	Inward Stroke.		
	inches.	sq. in.	inches.			lb.	lb.						
A. Rigg ..	16	201	24	180	1÷7	900	4·5	49·5	7·0	56·5	42·5	3 to 6	147
„ ..	16	201	24	200	1÷7	900	4·5	61·2	8·7	69·9	52·5	7 to 10	84
Porter-Allen	11½	103·8	20	230	1÷6	273	2·6	39·4	6·5	45·9	32·9	11 to 14	44
„ ..	11½	103·8	20	230	1÷6	273	2·6	39·4	6·5	45·9	32·9	15 and 16	70
A. Rigg ..	14	154	21	154	1÷7·4	565	3·66	25·9	3·5	22·4	29·4	17 to 20	104·7
Porter-Allen	11	95	16	350	1÷6	—	2·5	69·5	11·5	81	58	21 to 24	85
„ ..	11	95	16	350	1÷6	—	2·5	69·5	11·5	81	58	25 to 28	133
„ ..	12	113	24	200	1÷6·16	470	4·15	57	9·25	66·25	47·75	—	—
„ ..	14·5	165	24	200	1÷6	529·5	3·206	43·6	7·2	50·8	36·4	—	—
A. Rigg ..	12	113	20	130	1÷7	407	3·6	17·3	2·5	19·8	14 8	—	—
Column 1 ..	2	3	4	5	6	7	8	9	10	11	12	13	14

Table II. gives a summary of data which are intended to serve for calculations as to the influence of reciprocating parts, initial or mean pressure, compression and load or resistance; and a few introductory remarks on this table may perhaps prove useful. Data are given for several sizes of horizontal engines made by the Southwark Manufacturing Company of Philadelphia, and known as Porter-Allen engines; and also for several sizes of high-speed engines constructed by the author of the present paper; but it has not been considered necessary to include other classes of engines, as so few of them can be said to make a complete use of the reciprocation of their moving parts for gaining steadiness in running. No special explanation will be needed for the first five columns of Table II. Column 6 gives the ratio of crank radius ÷ connecting rod, in a fraction whose numerator is always unity. Column 7 has the actual weights of the reciprocating parts—viz. piston, piston rod, motion block, and the connecting rod, except its outer or crank-head end, as that may fairly be regarded as being part of the crank and balanced by a counterweight. Column 8 is obtained by dividing the figures in column 7 by the piston area, column 3, thus giving the weight of reciprocating parts per square inch of piston area, bringing all calculations into uniform terms, and so facilitating comparisons between different engines.

Column 9 is a calculation of the initial pressure necessary to put the reciprocating parts into motion. If an infinite connecting rod were used, then this initial pressure would exactly equal the centrifugal force, supposing the mass concentrated round the crank-pin.

Column 10 takes into consideration the actual finite length of the connecting rod (column 6) as compared with the corresponding crank radius, and expresses how much pressure must be added to, or deducted from, the mean initial pressure, on account of variations due to a finite connecting rod.

Column 11 expresses the actual initial pressures required for acceleration or retardation of the reciprocating parts at the end of cylinder furthest from the crank.

Column 12 gives corresponding data to column 11 for the cylinder end nearest the crank.

Great divergencies will be noticed in the proportions of these engines in accelerating pressures required, and in their weights of reciprocating parts relatively to their piston areas; and it will be apparent that no uniform rule can have been observed during their design in relation to such strains as arise from reciprocation.

The necessity for providing sufficient initial steam pressure is pretty generally understood, and the author does not intend

to notice this matter in the present paper, but less consideration seems to have been devoted to those heavy strains which arise at the conclusion of each stroke, and which sometimes convert a steam engine into a steam hammer, working its own destruction. It is to a consideration of these final pressures that the author proposes to apply and extend the circular pressure diagrams, so as to render them easily calculated, and to show at a glance how much resistance is needed for retarding the reciprocating parts, and for bringing them quietly to rest. There are three elements by which such resistance can be offered, namely:—

(*a*) The load upon an engine.

(*b*) Compression of exhaust steam (and lead).

(*c*) Impact on the crank-pin.

And of these three, impact on the crank-pin alone tends to act destructively, and must be limited by lead and compression within such degrees as may be found practically convenient in any given type of engine.

(*a*) The load or resistance on an engine may be considered as a uniform tangential pressure against the movement of its crank, and is measured upon the piston as the mean pressure given by an indicator card. Measured on the crank, this pressure diminishes in the ratio of two diameters to a circumference, thus:—

$$\text{Mean tangential load on crank} = \frac{2 \times \text{mean pressure}}{3{\cdot}1416}$$

It is only the horizontal component of this pressure that can oppose a resistance against corresponding horizontal pressures due to reciprocation; and if the circle marked I. (A C B D, Fig. 6, p. 93) be the path of a crank, then radial ordinates may be drawn from it as a base line, and all their terminations will be found in the circle marked II. as the exact equivalents of such horizontal component at any desired spot. At half stroke, when the crank is moving horizontally, this component exactly equals the mean resistance; but at either end it vanishes altogether, and so becomes quite ineffectual as a means for checking the movements of a piston.

(*b*) Compression of the exhaust may be taken to include lead, or inlet of fresh steam before a crank turns its centre; for both these are effectual against the final impact of reciprocating masses, after the resistance of load becomes feebler or ceases altogether; by compression any desired pressures can be accumulated against a piston for bringing all the reciprocating masses, however great, to rest, and this cushioning of an elastic vapour to a pressure approaching or even exceeding that of

incoming steam, serves also important economical results in the working of steam engines.

(*c*) Impact on a crank-pin results from whatever final difference remains between the whole sum of accelerating pressures, and the deductions that have been made from it by counteracting load or compression, and it should be the object of a design to limit this pernicious action within the bounds that surfaces can endure without expelling the lubricant, or, better still, to eliminate such impact altogether. In the indicator cards illustrated, and in all other diagrams, the line A B is taken for the piston stroke, and the vertical scale of pressures is always $\frac{1}{60}$ inch to the lb. per square inch of piston area. A P is the pressure per square inch for acceleration or retardation at the outer or further end from the crank; while B Q is the corresponding pressure for the inner end nearest to the crank; and all the engines described are horizontal. Ordinates from A B to P Q (Fig. 4, p. 93) represent deductions and additions for the forward stroke on account of reciprocation, while B *q* (Fig. 5) represents deductions and additions on account of the backward stroke. Finally, the curves F *f* (Figs. 4 and 5) combine actual indicator pressures with those deducted or added by reciprocation, and give final pressures on the crank-pin in standard measure of pounds per square inch upon each piston.

Circle I. in all diagrams such as Fig. 6 represents the path of a crank to a scale of three-quarters of an inch to the foot; and this circle is used as a base line for measuring all pressures and resistances. Circle II. gives the horizontal component of load, or uniform resistance, due to the work which each engine is performing, measured at any point as a radial ordinate to a scale of $\frac{1}{60}$ inch per square inch of piston area. Circles III. are the curves P Q and *q p* translated into radial ordinates upon the circle A C B D, as a base line, and they show graphically how much pressure is needed, at any given position of the crank, to counteract the influences of reciprocation, A P being for the extreme outer end, and B Q for the extreme inner end, thus corresponding with the indicator diagrams.

Portions of crescents are cut off between circles II. and III., and C E B shows exactly how much of the reciprocation may be counteracted by horizontal components of the load or resistance, and E Q B shows how much remains to be dealt with by compression or impact, or both. Similarly, at the termination of a return stroke, D *e* A is a resistance offered by load, and *e* P A remains over to be dealt with by compression of exhaust and impact against the crank. This opens up the question of valve setting, and its importance cannot be exaggerated, for upon

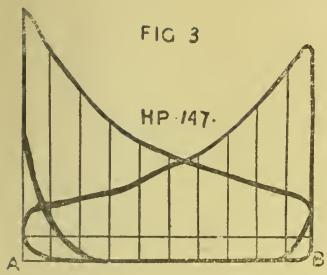


FIG 3
HP 147.
A
B

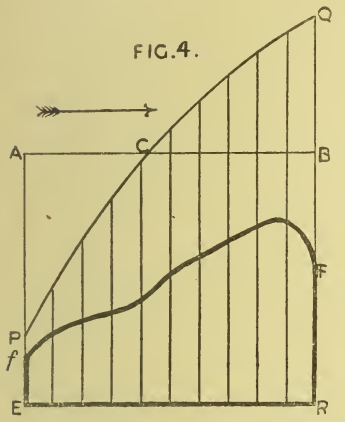


FIG.4.
Q
A
C
B
F
P
f
E
R

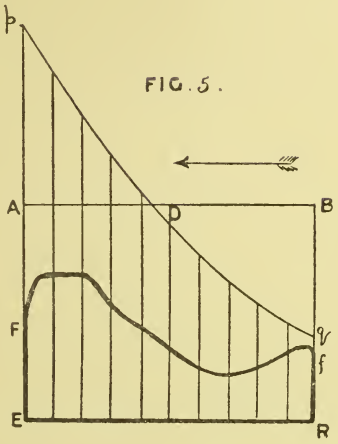
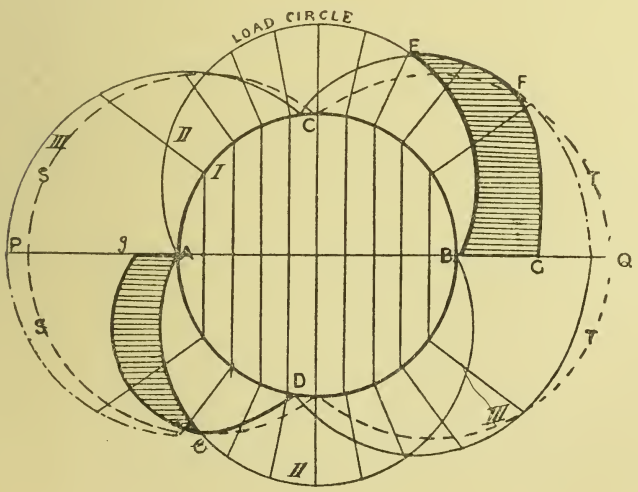


FIG.5.
p
A
D
B
F
v
f
E
R

FIG.6.


LOAD CIRCLE
E
F
C
III
II
I
S
P
g
A
B
G
Q
S
D
e
II
III

correct proportions of clearances and compression depends the quiet easy running and durability of any given high-speed steam engine. Compression curves taken directly for such indicator diagrams as Fig. 3 have been translated into their equivalents in circular diagrams, such as Fig. 6, and appear there as irregular crescents, measured from circle III. as base lines. Thus F Q G corresponds with the termination of a forward stroke, and *e* P *g* corresponds with the termination of a return stroke. These have to be deducted from the reciprocating pressure circles III., so that finally there are left areas E F G B and *e g* A, which are shaded by horizontal lines, these representing the duration and amount of whatever strains have to be resisted by the crank, and tend to its destruction.

The area enclosed by E F G B (Fig. 6) gives the duration and amount of pressure accumulated at the termination of the forward stroke, and *e g* A represents corresponding times and pressures for return stroke. Having secured all this information, it is easy to calculate the strains to which any given engine is subjected, and to understand why one may work well and another indifferently.

The author's first illustration is taken from a powerful horizontal condensing engine made by him in 1877. It has a cylinder 16 inches in diameter, 24 inches stroke, and drives its double-acting air-pump directly from an eccentric on the main shaft. This engine was designed to run at 160 revolutions per minute, but its regular speed was altered to 180 revolutions per minute at the commencement, and circumstances required a temporary increase to 200 and 220. Advantage was taken of these unusual opportunities to study the behaviour of this engine, at that date the most powerful ever constructed to run at so high a speed, and it was found to work well and quietly at 180 revolutions; but that the bearings were insufficiently large for speeds of 200 and 220 revolutions, and were difficult to keep cool, otherwise the engine ran well even under this severe test. The total weight of its reciprocating parts, exclusive of the crank end of its connecting rod, was 900 lb., equivalent to 4·5 lb. per square inch of piston area. Fig. 3 represents indicator diagrams taken from both ends of the cylinder at 180 revolutions, giving 147 horse-power, or a mean pressure of 33·6 lb. per square inch on the piston. In Figs. 4 and 5, we see curves P Q and *p q* portraying the necessary deductions and additions on account of reciprocation; while crank pressure diagrams are distinctly shown by E *f* F R (Fig. 4) for the forward stroke, and by R *f* F E (Fig. 5) for the return stroke from the crank backwards.

The load or uniform tangential resistance on the crank

FIG. 7.

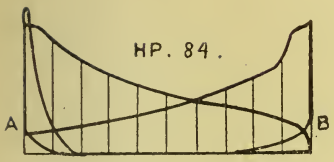


FIG. 8.

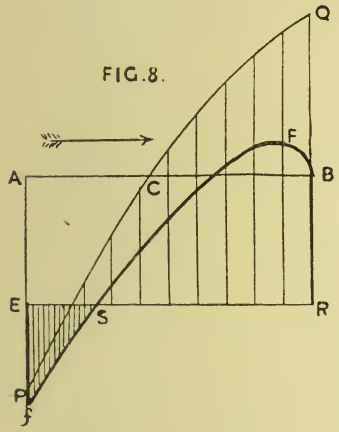


FIG. 9.

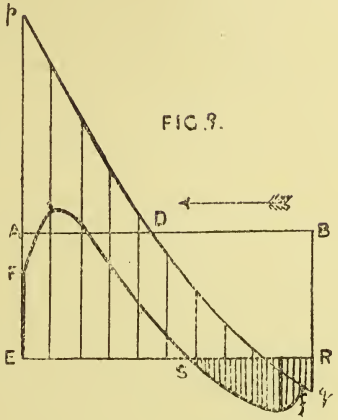
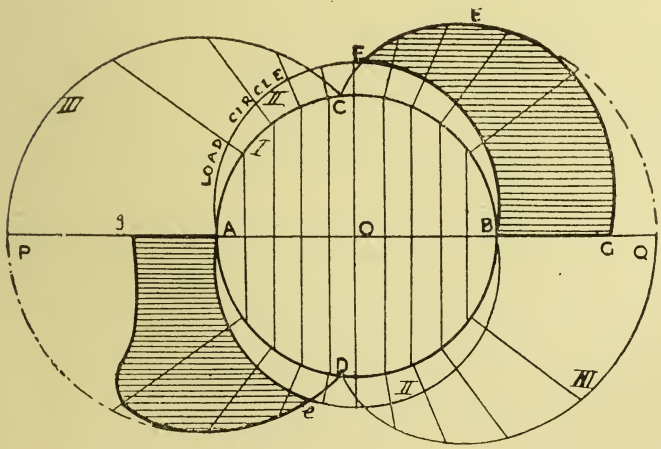


FIG. 10.



is 21·4 lb., and its horizontal components may be taken from radial ordinates between circles I. and II. The reciprocation curve P Q from Fig. 4 changes into the double crescent P C Q (Fig. 6), and similarly the reciprocation curve *q p* (Fig. 5) changes into the corresponding crescents Q D P. S S and T T are dotted to show what would be the reciprocating curves corresponding with a connecting rod of infinite radius. The horizontally shaded portion in Fig. 6 represents duration and amount of impact upon the crank-pin, after deducting resistances due to load and compression; and in the present example the crank moves through 108° in 0·1 second; simple measurement, therefore, shows that the forces of impact per square inch of piston are at—

Termination of forward stroke; pressure 20 lb., duration 0·05 sec.
„ „ backward „ „ 15 „ „ 0·05 „

We now take the same engine making 200 revolutions per minute, and doing 84 horse-power. Its indicator diagrams are given in Fig. 7, and the influences of its reciprocating parts are shown by Fig. 8 for the forward stroke and Fig. 9 for the return. Here it is not until one-fourth of the forward stroke has been passed and until one-third of the return stroke has been accomplished that sufficient steam pressure is forthcoming to balance the resistance given by inertia of its piston, &c.

Fig. 10 shows what is occurring at the termination of each stroke. A very feeble horizontal resistance is offered by the load circle, and the amount of compression proves quite inadequate for bringing the rapidly moving masses to rest. Under these conditions the engine could not possibly work in the quiet and easy manner which its slower speed of 180 and its higher power rendered apparent. Here the crank moves through 120° in one-tenth of a second, and the impact forces are—

For end of forward stroke; pressure 37 lb., duration 0·06 second.

For end of return stroke; pressure 33 lb., duration 0·04 second.

In order that this engine shall work well and quietly at 200 revolutions, the initial pressure should be increased to not less than 80 lb. per square inch, the load about doubled, and the exhaust closed much earlier to provide sufficient cushioning that may bring about a state of rest at the end of each stroke.

Figs. 11 to 16 illustrate the running conditions of an $11\frac{1}{2} \times 20$ inch Porter-Allen engine making 230 revolutions per minute, and producing 44 horse-power with diagram,

FIG. 11.

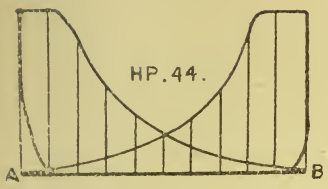


FIG 12

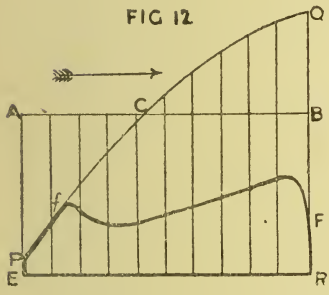


FIG. 13.

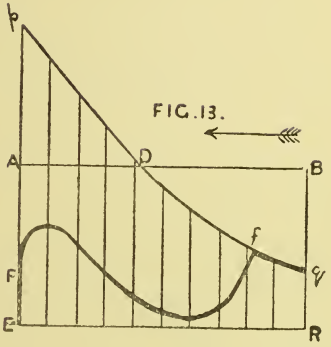


FIG. 14.

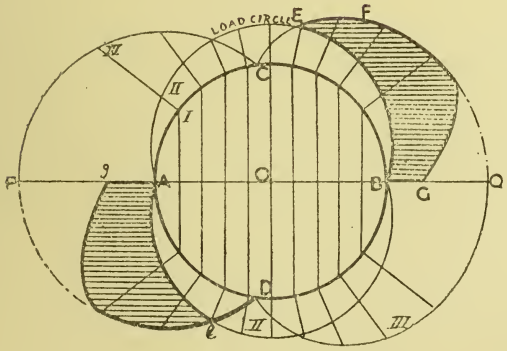


Fig. 11, or giving 70 horse-power with diagram, Fig. 15. The ± pressures due to reciprocation are given by Figs. 12 and 13, and though they show no absolute deficiency of propelling pressure, yet its margin is perilously low at four-tenths of the return stroke, and although a fair average resistance is given by the load, yet the compression is insufficient.

By methods already explained the circular diagram, Fig. 14, is constructed and measured. It shows that the mean impact on its crank in terms of lb. per square inch—

For end of forward stroke; pressure = 18 lb., duration = 0·05 second.

For end of return stroke; pressure = 25 lb., duration = 0·04 second.

If we now turn to the indicator cards, Fig. 15, and corresponding circular diagram, Fig. 16, these show the same engines

FIG. 15.

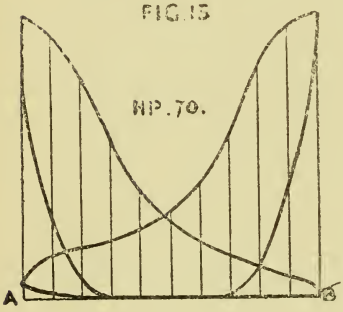
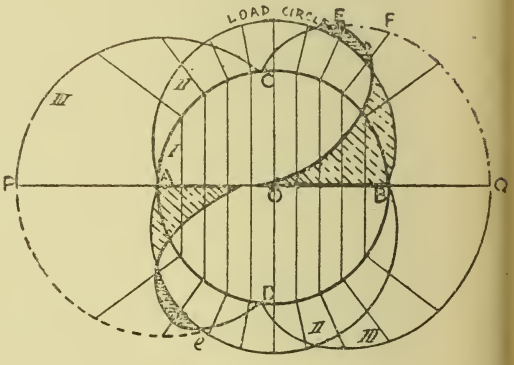


FIG. 16.



still making 230 revolutions per minute; but a very great change has come about. The forces of impact have practically disappeared, and have been actually reversed by an excessive compression shown on the indicator cards. This remarkable illustration shows how completely the dynamical relationships of high-speed steam engines are under the control of the engineer, when once their principles have been thoroughly investigated and understood.

Figs. 17 to 20 inclusive are taken from a very successful high-speed engine made by the author, having a cylinder 14 inches in diameter, a stroke of 21 inches, indicating 104·7 horse-power, and making 154 revolutions per minute. The degree of compression was very carefully proportioned to the

FIG. 17

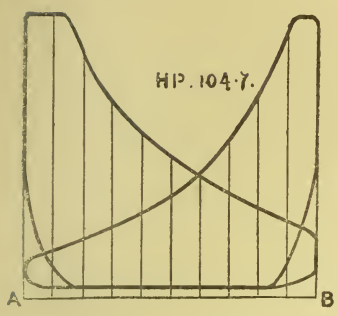


FIG. 18.

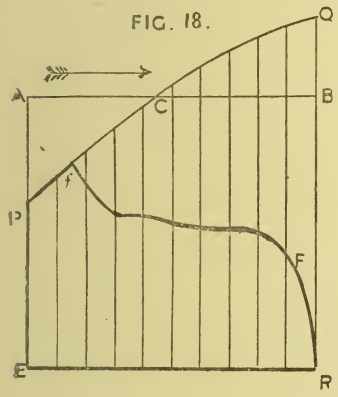


FIG. 19.

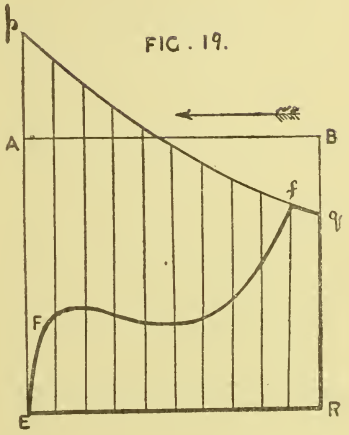


FIG 20

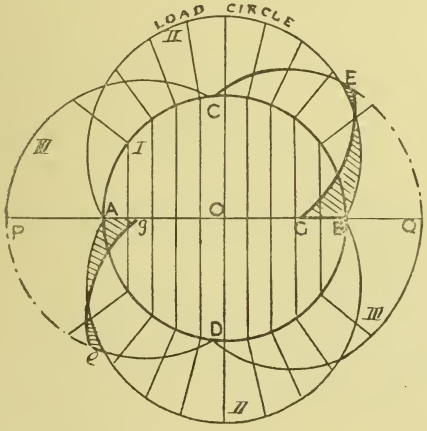


FIG. 21.

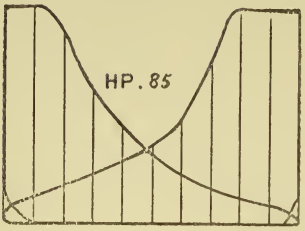
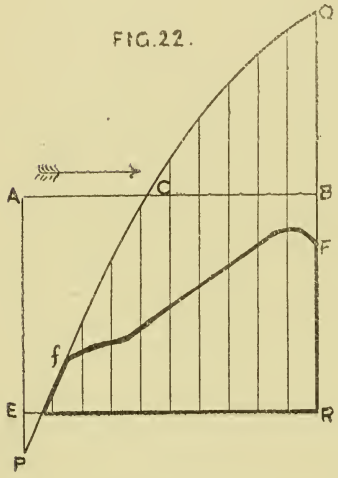


FIG. 22.



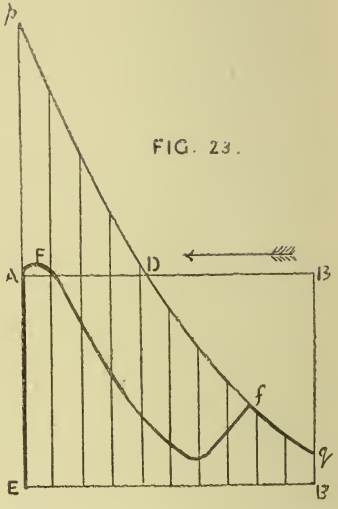
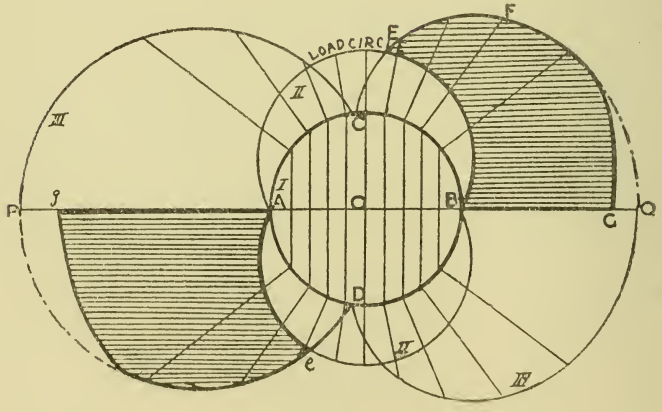


FIG. 24.



weight of reciprocating parts, and therefore Fig. 20 shows an absence of those destructive forces so conspicuously present in Figs. 10 and 14. This satisfactory result has been accomplished without overdoing the compression as in Fig. 16, and notwithstanding its heavy load this non-condensing engine has worked regularly since 1880 in a satisfactory manner, and moreover with marked economy in fuel.

Of late years an opinion seems to have grown up, that because small high-speed engines can run with satisfactory results at speeds exceeding 200 revolutions, therefore the same engines may be driven far faster with equal success. This opinion has partly been fostered by the requirements of electrical engineers, and has led to a series of disasters of a character most discreditable to our national reputation for engineering skill.

No data are forthcoming by which graphic diagrams, similar to those we have already seen, can be constructed for innumerable agricultural engines that have broken down under the absurd conditions of their work. Similar severe tests have been given to American engines of a far stronger and more scientific construction, and an example is given by Figs. 21 to 28 inclusive, from an 11 inch by 16 inch Porter-Allen engine making the extremely high speed of 350 revolutions per minute, and driving a dynamo direct. Fig. 21 represents indicator cards showing 85 horse-power, and Figs. 22 and 23 are corresponding crank pressures, transferred as portions of circles marked III. in Fig. 24. Here the forward stroke (Fig. 22) receives insufficient pressure for giving motion to its reciprocating parts until one-tenth of its travel has been accomplished; and the final strains also are irregular and extremely heavy.

Under such conditions it would be surprising to find the engine working easily, and the construction of a diagram, Fig. 24, shows an excessive strain coming on the crank-pin:

For the end of forward stroke; pressure = 40 lb., duration = ·05 second.

For the end of backward stroke; pressure = 52 lb., duration = ·025 second.

The indicator cards, Fig. 25, show the same engine still making 350 revolutions per minute, but doing 133 horse-power. Here the crank pressures become more regular, the load counteracts a greater proportion of the impact strains, but the amount of compression remains wholly inadequate to produce quiet and easy running. The remaining pressures are as follows:

For the end of forward stroke; pressure = 32 lb., duration = ·03 second.

For the end of backward stroke; pressure = 44 lb., duration = ·025 second.

FIG. 25

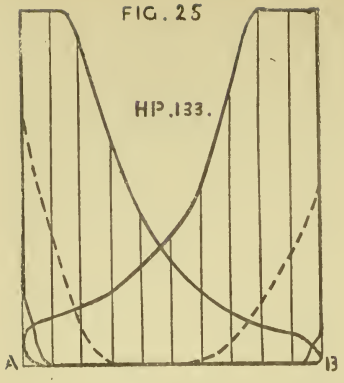


FIG 26

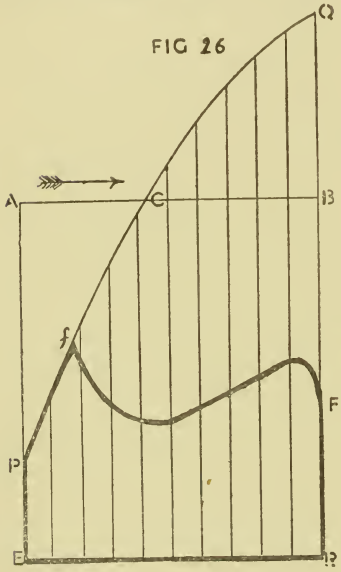


FIG. 27.

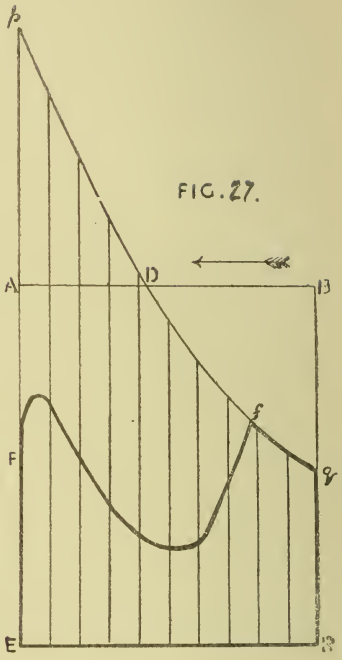
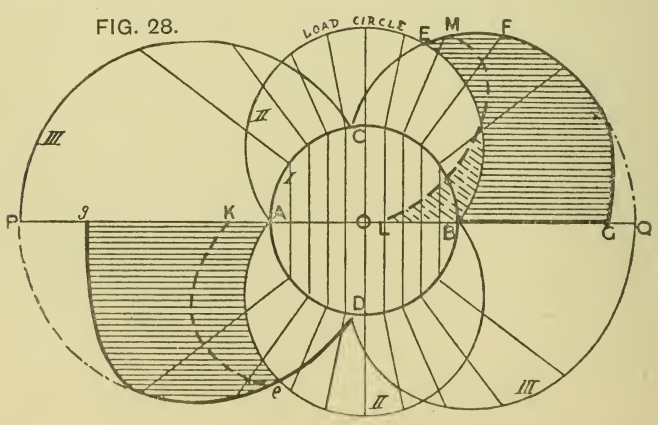


FIG. 28.



In neither of these cases can this engine be expected to run well, and the means of cure are obvious from these diagrams at a glance.

By way of further illustration it may be assumed that this engine has its valves reset so as to increase the amount of compression to that shown by Fig. 25, and for our present purpose we may neglect the increased mean pressure thus required to maintain 350 revolutions and to give out 133 indicated horse-power.

The results of this change are shown by additional curves drawn upon Fig. 28 and terminating at K and L. Here compression at the end of a forward stroke produces everything that can be desired, but a little additional compression might advantageously be employed at the termination of a return stroke.

The curve terminating at L (Fig. 28) may be compared with those terminating at G and *g* in Fig. 20, for they all fulfil some of the conditions of perfectly quiet running, namely, that any forces given out by the reciprocating parts towards the conclusion of a stroke, as shown by shaded portions, shall be met and counterbalanced by something more than an equivalent resistance being required for compression. This margin ensures relief from an evil which accompanies high-speed engines, namely, irregular load.

Reverting to Fig. 20, and assuming the load circle II. to approach closely to the base circle I., then, if an automatic governor be used to vary the rate of expansion, we should find that diagrams like 8 and 9 would be produced, where there is not enough steam pressure to give the necessary acceleration, and we should also find that the points E and *e* would closely approach *c* and D, thus leaving a large margin of unresisted forces to become a destructive impact upon the crank-pin.

Such diagrams as these afford an unerring guide to the designer of any high-speed steam engine; for, after deciding upon its dimensions, speed and intended range of power, such diagrams as Fig. 28 can be constructed so far as circles I., II., and III. are concerned; then radial ordinates can be transferred from this preliminary or trial diagram back to indicator cards like Fig. 25, where their terminations will display compression-curves such as are shown by dotted lines on that figure. It also follows, as a matter of course, that similar investigations may be taken from any existing engine in order to discover the proper place where compression should begin. And if the designer cares to be perfect in every way, he can thus very readily find out how much clearance spaces should be allowed. It is a common error to imagine that the reduction of such spaces to a minimum is an unmixed gain in the case of high-

speed steam engines, nor should it ever be alike at both ends of a cylinder.

Judging by these examples, all taken from engines in actual work, it cannot be a matter of any surprise that contradictory results are obtained from high-speed engines. Indeed, there are many other engines not avowedly of a high-speed character, but often of considerable size, and these give immense trouble through heating of bearings and other pernicious habits.

If the engineers in charge would only take the trouble to weigh the heavy masses which are kept in comparatively rapid motion; if they would select indicator diagrams, and lay bare the whole secret history of their charge in one revolution of its existence, by the use of these new diagrams which the author hopes may prove interesting and useful to his fellow engineers; then there would be no difficulty whatever in ascertaining what changes are necessary to remove the evils experienced so frequently, the working days of their engine would be passed in quietude, its life prolonged, and their own anxieties in no small measure diminished.

DISCUSSION.

Mr. SCHÖNHEYDER said that for many years he had taken into consideration the effects of the motion of the reciprocating parts of engines, though not necessarily of high-speed engines, for evil results might arise if they were not taken account of, because some very slow speed engines had very heavy parts. He quite agreed with the author with regard to the diagrams generally, but the torsion-diagrams on the wall seemed to have the defect that, while the stresses upon the crank-pins could be measured upon them, the figures did not represent the amount of work of acceleration. He always preferred what might be called a flat diagram, like the indicator diagram, with the addition of lines representing the effects of the reciprocating parts. By the use of this they could measure not only the stresses on the crank-pin, or other working parts, at any portion of the stroke, but they might take the areas representing the excess or deficiency of energy, and from them calculate the necessary weight of the fly-wheel, or, if they had the weight of the fly-wheel, calculate the increase and decrease of speed at any portion of the stroke. He did not see how it was possible to do that from the circular diagrams which Mr. Rigg used. He was at present engaged in designing some very heavy pumping engines which would work at only twenty revolutions per minute, yet he was obliged to take very careful account of the weight of the reciprocating parts. These were so heavy that, instead of the load being uniform through-

out the stroke as one might suppose, the pressure which the steam had to overcome at the beginning was about three times that which it had to overcome at the end.

Mr. W. Worby Beaumont said that he did not think that the diagrams took into consideration the effect of the work that had to be done by the crank of a horizontal engine running at a high velocity, in moving the connecting rod in a direction transverse to the direction of movement of the piston. They considered the effects of inertia as regards the movement backwards and forwards or in the same direction of movement as the piston, but, so far as he could see, they did not take into account that which had to be done, of course, by the steam through the medium of the crank-pin in lifting the connecting rod, or even in throwing it down with a velocity greater than that at which gravity would take it. With engines running at the speed which Mr. Rigg had mentioned, the connecting rod had to be moved up and down at a great rate, for the greatest velocity in a transverse direction had to be imparted in the time that the crank-pin was making one-fourth of a revolution, and that must have very considerable effect and might perhaps explain the breaking of the connecting rods in some very high speed engines, of which they sometimes heard. Another point which he should like to mention was the idea that Mr. Rigg apparently had that engines which were not satisfactory seemed to be always introduced by agricultural engineers. He (Mr. Beaumont) thought that some of those who used to be known as agricultural engineers had done more than most engineers to make satisfactory engines up to 20 nominal horse-power. He believed it was true that even to the present time one firm of agricultural engineers had produced more engines for long steady driving of electric-lighting machinery than any other firm in England. No doubt at the agricultural shows there would be found, under the name of steam engines, a great many things which Mr. Rigg would not call steam engines, but for which he, and others too, would find some other name; but Mr. Rigg must admit that some of the engines made by agricultural engineers were equal to any that had ever been made. He did not think that there was anything which need be said concerning the diagrams, because he supposed that most engineers who had anything to do with the construction of engines were acquainted with Mr. Rigg's book on the steam engine, and he believed that almost all that was shown in the present diagrams was explained at length in that book; and if engineers had not at present understood that to which the paper seemed chiefly to lead—namely, that they could do

almost as they liked in producing satisfactory running in most engines, by finding out exactly what amount of compression, either one way or another, should be obtained—it was certainly their own fault, for Mr. Rigg's book was very clear, and, for that which was in it, it was also very cheap.

Mr. VAUGHAN PENDRED said that the author had spoken of "high-speed engines." One of the most essential features in a discussion was to have something like accuracy of definition. What did Mr. Rigg mean by a high-speed engine? So far as he could find out, Mr. Rigg referred to engines running up to 250 revolutions a minute. The number of revolutions was one thing, and piston speed was quite another thing. If they had an engine running at 30 revolutions at a given stroke, and they doubled the stroke, the effect on piston speed would be the same as doubling the revolutions. He thought that it would have been better if they had had a fuller idea of what Mr. Rigg really meant, for he (Mr. Pendred) did not think that an engine making 250 revolutions could be called a very high speed engine in the present day. He had himself seen ordinary double-acting engines with crank and connecting-rod running at 1300 revolutions, driving fans. If they took some of the great ocean steamers of the present day, they would find them with 6 feet stroke, making 72 revolutions a minute. This gave 864 feet of piston speed per minute—a tolerably high speed, although the revolutions were few. One of the most important points was to learn what was the special gain, in pounds, shillings, and pence, derived from using a high-speed engine. What was the special advantage in pounds, shillings, and pence derived from using a very large measure of compression? Such were the questions that people in the present day would ask. What was the first cost of the engine and what was the saving of coal, and how far the buyers' pockets were affected by adopting the principles set forth in the paper. Mr. Rigg had given them a very dreadful picture of what took place in engines which were not properly balanced, or properly compressed, and had not sufficient lead; but they found that the great majority of engines which had had no particular attention paid to the question of lead ran at tolerably high speeds and did very well. On the other hand, it would be an improvement to the paper, and of considerable interest to engineers, if Mr. Rigg would show what was the pecuniary return obtained from adopting those large measures of compression. His own conviction was that for every class of engine there was some measure of compression, and some pressure and speed, which were better than any other, and that no general rules could be laid down, based merely upon

the amount of reciprocating weight here and there. Turning to the Porter-Allen diagram, for example; as he understood the figure, the proper amount of compression for the engine would be shown by the dotted line on the diagram, so that the whole corner, from the dotted line downwards, would be cut off. He supposed that the engine had been indicating a certain power; he really forgot at the moment what the power was.

Mr. Rigg: 133 horse-power, 11-inch cylinder.

Mr. Pendred (resuming) asked whether they were to understand that the corner shaded on the diagram represented so much waste of power originally. He did not quite follow out the paper as to the great evils which plenty of lead was to compensate for, such as thumping, pounding, and hammering of crank-pins, and knocking brasses to pieces. Many engines were made which had very little lead, and yet worked very quietly indeed and at a very high speed; and he knew engines practically which had a very considerable amount of lead and which hammered and thumped very much. It would be generally found that one cause of this thumping was that the crank-pins were not round. There was an enormous number of oval crank-pins in the world, but they managed to get along. The great difficulty in carrying out Mr. Rigg's principle was that it was constant for every engine. Let them suppose that they applied Mr. Rigg's principle to an engine which was driving a dynamo, and was making 300 revolutions a minute. If they cut off half the work, or varied the work at all, it was essential that the engine should continue, as far as possible, to move at precisely the velocity at which it was moving before. It would do so within 1 or 2 per cent. But if the engine had been properly compensated, on Mr. Rigg's plan, for one speed, it would be wrong for another. Therefore the principle did not admit of universal application. It appeared that the use of a governor was absolutely inimical to the carrying out of Mr. Rigg's views. They would want an absolute boiler pressure and an absolute load, and an absolute point of cut off, and so on, in the engine. It would be a matter of considerable interest if some one present would show whether it was not possible to have a valve gear which would compensate to a large extent for the errors (as he might call them) introduced by the action of a governor. They had it to a certain extent in a locomotive with the link motion. When the engine was running at slow speeds it had a very full diagram, but there was little compression; but as the speed got up and the engine was linked up, it would be found that the compression increased at the same time. Mr. Rigg had spoken about himself as the first to produce a very high speed engine at a certain date; but he

(Mr. Pendred) fancied that the locomotive might be allowed to have anticipated Mr. Rigg's engine as a high-speed engine.

Mr. B. Crichton said that it had always been a matter of considerable trouble to him to obtain decent diagrams from engines running at high speeds, such as 400 or 600 revolutions a minute, and 800 to 1000 feet per minute piston speed. He should like to ask Mr. Rigg in what way he obtained his diagrams, and also what kind of valve was used in the Porter-Allen engine. He should think that Mr. Rigg's method of balancing or compensating engines would be likely to be of some value to engineers, but he was afraid that but few engineers would have time to go into the necessary calculations of weights, &c. Mr. Rigg had not entered into the subject of balance-weights at all. He (Mr. Crichton) had found that properly proportioned balance-weights tended to make an engine run tolerably steady and free from vibration. He questioned whether much better results would be got by Mr. Rigg's method, as vibration from centrifugal forces and from the oscillation of the connecting rod was not provided against, as was done where balance-weights were used.

Mr. Druitt Halpin said Mr. Pendred had stated that Mr. Rigg's system would be applicable were it possible to provide a system of valve gear in which the compression was varied. He took it that the essence of the Armington-Sims, as of all that class of valve gears, was to produce this effect. By the use of two eccentrics, one within the other, the valve-travel was altered, the compression being proportionately changed, while the lead was maintained constant. As to the taking of diagrams, he thought that it was quite possible in the earlier engines to use the Richards indicator, where the speed was limited to from 150 to 175 revolutions; but the new American indicators, like Crosby's or Tabor's, could run comfortably up to 500 or 600.

Mr. Rigg said that one object he had in bringing forward the new diagram was to show how, with a few minutes' labour, any engineer could ascertain how much compensation was required to secure the easy running of any particular engine. Mr. Schönheyder had drawn a comparison between circular and flat diagrams; but although the latter showed areas to the eye with correctness, they did not show time, and therefore did not faithfully illustrate force. But exactly the same argument would apply to an ordinary indicator diagram, showing as it does pressures correctly enough, but giving a very imperfect appreciation of time, to such an extent, indeed, that ·15 from each end of the stroke, or ·3 altogether, occupies just as much time as the remaining or intermediate ·7 of the stroke. When

investigating these matters it was necessary to choose whether the eye should appreciate times or appreciate pressures, and it might probably be convenient to make two diagrams, one showing times and one pressures.

Mr. Beaumont had drawn attention to the fact that the new diagram took no consideration of the vertical reciprocations of the connecting rod in a very rapidly moving engine. Very important strains arise from the cause he pointed out, but it was absolutely impossible in the time allowed for a paper to consider all the strains that arise from rapid reciprocation. Therefore only one portion of the subject had been considered in the present paper. The point mentioned by Mr. Beaumont had a very important bearing on the running of high-speed engines, but he did not think that it properly entered in any way into a discussion of the forces for starting and bringing to rest any ordinary horizontal movements of the reciprocating parts. He was sorry that Mr. Beaumont should think that he (Mr. Rigg) had spoken of agricultural engineers as if they were the only unsatisfactory makers, for such was by no means the case. For its original purpose, driving agricultural machinery, the ordinary agricultural engine was sufficiently good, but for electric lighting nothing could be more unsuitable than to use such an engine. The better class of agricultural engineers had profited by experience, and they had unquestionably made improvements in their work. When electric light companies first commenced operations, they bought agricultural engines because they were cheap and portable, and put them to do work for which they were wholly unsuited.

In answer to Mr. Pendred's question, "What is a high-speed engine," his reply was that ideas as to high-speed engines were continually changing. What was high speed to-day would be slow to-morrow; and he believed that in time, instead of running pistons at hundreds of feet per minute, they might get to thousands. Mr. Halpin had answered Mr. Pendred's remark about variation of the diagram to compensate for the change of load. It would be very easy to construct a valve that would compress the exhaust exactly in a proper ratio, but that would be a refinement of practice which he was afraid it would be hopeless to expect, and which perhaps it would be hardly worth while to see carried out. As to the manner in which the diagrams were obtained from the author's high-speed engines, no suitable indicators existed at the time they were wanted; so he took an ordinary Richards indicator to pieces, re-made it with light moving parts, and so obtained the diagrams. The diagrams given for a Porter-Allen engine at 350 revolutions were taken by Crosby's or Tabor's indicators. Indeed, as

American engineers had paid more attention to high-speed engines than had been paid in this country, and with great success, so they had devoted a similar attention to indicators. The readiness with which American engineers had given him information for the present paper deserved his acknowledgments, more particularly as engineers at home are rarely so communicative as to the results of their experience.

Mr. BEAUMONT said that it seemed to him that if the double-acting engines are to be as unhappy as they evidently must be in some cases, as shown by Mr. Rigg's diagrams, the single-acting ones ought to be still more unhappy. He should like to know whether Mr. Rigg had considered the inertia effects in some of these engines, and whether it was not the case that some of the failures of which they heard during the Inventions Exhibition had been in single-acting and not in double-acting engines.

Mr. SCHÖNHEYDER remarked that it was quite possible to get "time" represented in the flat diagram as perfectly as it was in the circular diagram.

Mr. RIGG said that he had a great deal to say about single-acting engines. It was his intention to read a paper on the subject at the British Association at Birmingham. The single-acting engines had come very much to the front of late, but he did not wish to say more about them at present. Mr. Beaumont had said that single-acting engines must be so much worse off, because they had no cushion. Some makers, however, went to the trouble of making single-acting engines with an air cushion. He had seen it stated in print that it was a positive advantage to have a single-acting engine because it had no cushion. That statement was absurd, and was not true. Mr. Schönheyder had remarked that flat diagrams could be made to represent time. He (Mr. Rigg) was perfectly aware that in Armengand's work there were diagrams which represented time, but they were terribly distorted, and they had a sudden break, so that it was not easy to appreciate the time. They had to consider practical working engineers, who had had no mathematical education, and graphic diagrams were better suited for such persons, as they could understand them with but little study.

The PRESIDENT, in closing the discussion, said that the subject which had been before the meeting was one which Mr. Rigg had made his special study, and which most other people had not looked into so carefully as the author had. The paper had given a large amount of information of a useful and suggestive character. It remained for those who had not studied the subject to take it up from Mr. Rigg's point of view, and apply the results to their own special purposes.

May 3rd, 1886.

PERRY F. NURSEY, President, in the Chair.

INDUCED VERSUS FORCED DRAUGHT FOR MARINE BOILERS.

By W. A. Martin.

This paper is intended to bring before the Society the subjects of induced and forced draught for increasing the efficiency of marine boilers. Forced air and closed stokeholds have of late been brought into use and largely adopted for naval purposes, and it is but reasonable to suppose that much valuable information has been obtained by the steam department of the Navy.

By the introduction of iron and steel for shipbuilding, both the speed and tonnage of ships have increased considerably; improvements in marine engines have also received great attention, but the same cannot be said of the boilers upon which their success depends. They have been allowed to remain much as they were forty years ago, and so long as space could be found for them they were not considered. It was to meet the requirements of the Navy that attempts were made to increase the efficiency of steam generation by artificial means. This being a new departure it has caused great interest to be taken in it. The capabilities of fans for blowing or forcing fires have been known for years, but in adopting them for forcing air to marine boiler furnaces, it was found necessary to close the stokeholds, and make air-locks for entrances, so that the outlets should be through the fires, the pressure of air being regulated by the speed of the fans. The furnaces being no longer dependent on natural draught could be forced, and the rate of evaporation increased at will. To consume 15 lbs. of coal per square foot of grate per hour, with natural draught, is considered high, yet it is stated that 100 lbs. have been achieved by the forced air system, a rate that would greatly tax the capabilities of the stokers and the boilers also.

Forced air having been brought into requisition for marine boilers, opens up a wide field for discussion, especially as both mechanical and chemical laws are involved in it. The problem is how to make coals produce their maximum effect. So long

as marine boilers had to depend upon natural draught for a supply of air to the furnaces, only a fixed duty could be obtained from the coal. To produce higher rates, mechanical devices have to be employed. The vital question which arises as to what quantity of air is requisite per lb. of coal to burn it usefully, has not, as a rule, been taken account of on official trials. The chief points taken in them for measuring results are the weight of coal consumed per square foot of fire-grate, and the indicated horse-power per hour. As the results are so different with various boilers, it shows that some more definite data are needed. The rate of coal consumption will not define the indicated horse-power per foot of fire-grate on the forced air system. The rate in H.M.S. *Polyphemus* is, coal 48·14 lbs. per square foot of grate per hour, the indicated horse-power per square foot of grate 15·63; whilst in the *Howe* the consumption is only 33·47 lbs. to do the same duty; a difference of 14·67 lbs., clearly showing that some means are needed for regulating the rates of combustion. It is quite understood that economy has not been considered in adopting the system for naval purposes; these high rates are only intended for short runs. It is well known that considerable difficulties were experienced on its first application. In the case of the *Polyphemus* it involvedthe whole set of twelve boilers: well made as they were, they could not be made to stand forced air, and they had to be removed and replaced by others of a different class. These boilers were of the locomotive form, and if they had been worked on the locomotive principle would have been excellent steam-generators.

The locomotive is the highest example known for rapid steaming, due to the blast-pipe in the funnel producing induced draught and high temperature in the furnace; its full force can be used without injury to either boiler or tubes. The heat is drawn in regularly through the full length of the tubes as fast as it is generated, there is no delay or storing in the fire-box, or trouble with priming. Reverse the process by forcing air to the furnace, the result would be to convert the furnace into a battery, and the tube-plate into a target exposed to the full force of the heat generated. The resistance in the tubes also having to be overcome would cause the heat to impinge severely on their orifices. So with marine boilers. It is the tube-plate which suffers most, as there is a constant pressure against it, according to the force of air supplied.

There has been so much trouble experienced with boilers upon the forced air system, that numerous devices have been resorted to in order to obviate them. The combustion chambers have been made longer than usual, and the tube-plate placed

further from the furnace. In some of the French gunboat boilers an inverted brick partition has been built in the combustion chamber, in order to break the initial effect of the heat. By forcing air through the fire, a constant shower of dust and cinders is blown into the tubes and smoke-box, and large quantities up the funnel. So much trouble has been caused by this, that in most cases the pressure of air has been reduced considerably. Whatever might be said in favour of closed stokeholds, there is a feeling of insecurity in being shut up with boilers, with only air-locks for escape, if anything happens. There are no records of prolonged steaming at full speed, neither does that appear possible on this system, or that it can be made suitable for merchant ships.

Induced Air Current.

Induced air by mechanical means for increasing the efficiency of combustion is an old idea and was tried on land boilers before 1854, and more recently on marine boilers. It has been tried on the torpedo boat *Vesuvius* and it is stated that the results were unfavourable, but this may have been due to the defects in the arrangements, as the means employed for forcing are equally available for inducing air if properly adapted to the service required.

The solution of the problem of obtaining increased draught without the destructive effects and the dangers of the forced air system is, in the author's opinion, to place the fan at the extreme exit of the furnace and exhaust by its means, instead of placing it in the stokehold at the furnace entrance and blowing into it. The appliance by which the author accomplishes this result consists of a pair of fans (Fig. 3) placed in the base of the funnel, one on each side of the uptake, and mounted on a shaft carried through the uptake in an iron casing. The uptake is fitted with a damper which is open, as shown by the dotted line, when steaming with a natural draught. On closing the damper, however, the products of combustion coming through the boiler tubes into the uptake, are deflected through the fans, and follow the course indicated by the arrows. The high speed imparted to them by the fans induces a correspondingly accelerated draught through the whole of the tubes and furnace passages. It will be seen that the draught is thus made entirely independent of the height of the funnel, which, indeed, may be dispensed with altogether.

To test the practicability of this, the author obtained a marine boiler of modern construction and set it up in his factory, so that it might be worked on the same conditions as

in a vessel, and extended trials made without interruption. The results have shown that the duration of steaming with induced air may be continued indefinitely, or for the longest voyage a ship can make.

The following are the particulars and dimensions of the experimental boiler and fans:—

		ft.	ins.
Steel marine boiler	Length	6	0
" " "	Diam.	5	6
Single flue	Diam.	2	3
Tubes No. 44	Length	4	6
" "	Diam.	0	$2\frac{3}{4}$
Area of fire-grate	6·75 sq. ft.		

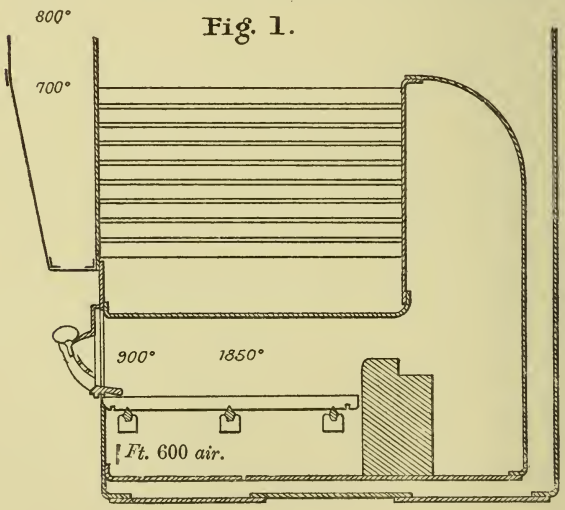
Heating surfaces—

Tubes	97 sq. ft.
Furnace	17 " "
Combustion chamber	38 " "
Total	152 sq. ft.

Heating surface per square foot of fire-grate = 22·5 sq. ft.
Fans .. Diam. 24 in.
Diam. of inlet .. 12 "
Revolutions per minute = 1150.
The fans used have flat blades.

Fig. 1 is a longitudinal section of an ordinary boiler,

Fig. 1.



working with a natural draught, and upon it are marked the average temperatures in the fire, the tubes, the smoke-

box and the funnel. The average rate of draught is also shown.

Fig. 2 shows a longitudinal and a transverse section of the boiler of a French gunboat, designed for working by forced air. It will be seen that an inverted arch has been introduced in the centre of the combustion chamber. This addition has been found to be absolutely necessary to moderate the destructive action of the fire, when driven by the blast from the fans direct upon the tube-plate. The accumulated pressure in the combustion chamber, as it endeavours to force its way into the tubes against the resistance of the gases in them, acts most violently on the orifices of the tubes themselves, soon causing leakage. It must be remembered that the pressure of the blast from the fans is constant, while the condition of the furnace, as to its power of usefully taking up the forced air, is always changing by the burning of the fuel, and not only is this the case generally over the fire-grate before and after a charge, but at any one time the condition of the fire-grate at various parts differs greatly, some portion or other being always more or less bared of fuel.

Again, as so much more coal is consumed on this system, the disturbance due to the opening of the fire-doors is greatly aggravated, and with the increased coal a corresponding increase of clinker is deposited on the fire-bars, rapidly closing the air entrances, so that the combustion is chiefly on the surface of the bars instead of on the surface of the fuel.

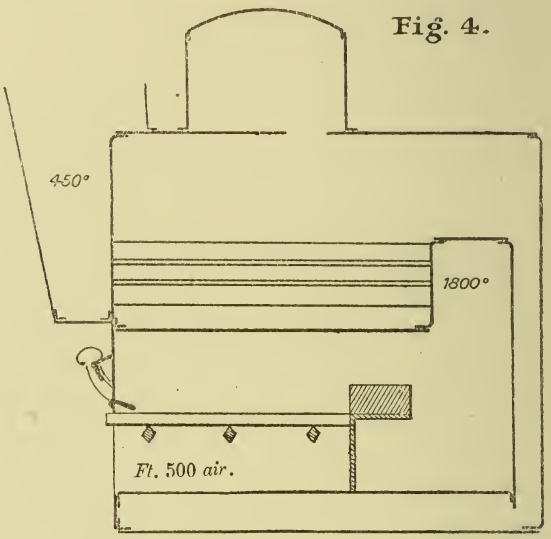
Fig. 3 shows the author's arrangement for the induced draught, with the fans and their mounting on the top of the uptake. It will be seen that a larger proportion than usual is given to the inlet of the fan. This is required on account of the expansion of the gases rendering larger inlets necessary than are required for cold air. Provision is also made for preventing the shaft which passes across the uptake from transmitting the heat it may there acquire to the bearings at either end. Not only is the centre portion of the shaft protected by a sleeve, but it is not made continuous through to the bearings, but a pair of hollow couplings are interposed between it and the bearings, by means of which continuity of temperature is broken, and the bearings are found to run perfectly cool.

The driving gear was also an important question. Special trials were made in October last, to test the capabilities of that adopted for driving a pair of 3-foot fans. These experiments were witnessed by several engineers and by an Inspector

from the Admiralty. The force on the inlet side indicated 15 lbs. per sq. foot over a 4-foot funnel, after which it was run daily to test endurance. Brake power was also applied to the fan shaft in addition to the fans, yet no slip could be detected or any perceptible wear, and as the wheels run in the same direction the friction is reduced to a minimum.

Fig. 4 is a longitudinal section of the experimental boiler used by the author, arranged for the induced air draught.

Fig. 4.



The temperatures are shown at corresponding points to those given in Fig. 1, and the amount and rate of draught. Other particulars are given on Table II. and were taken on the 13th April, when the Admiralty Inspector was present at an eight hours' trial. The results may of course be varied according to the rate given to the fans.

The experimental boiler was put down early in March and has since then been daily at work supplying the steam for the author's works, thus affording daily opportunity of trying various experiments, the fans having been fixed from the first, as a permanent thing. Special apparatus has been in use during the experiments for taking the temperatures at various points, measuring the cubic feet of water evaporated per lb. of coal, and per foot of fire-grate. The author considers that

these latter are more certain indications of efficiency than the horse-power per foot of grate which is usually recorded.

In comparing the three systems, of ordinary draught, forced draught, and the author's induced draught, as shown on the accompanying illustrations, it is necessary to say that the temperatures and draught have not been given for the forced draught boiler, because no information on the subject is obtainable. It may, however, be noted that in Mr. F. C. Marshall's boiler trials (of an hour and a half) with forced draught, he records temperatures averaging for the stokehold 76°·50 and for the uptake 1180°. In this case also the water evaporated per lb. of coal was 8·56 lbs., from and at 212°, and the coals per square foot of grate were 118·1 lbs. Compare these results with those obtained by induced draught—a temperature in the uptake of 450°, water 12·25 lbs. per lb. of coal, and 21·92 lbs. of coal per square foot of grate.

The enormous temperature in the uptake, under the forced air system, is of the highest importance, not only on account of its danger, but as proving how little duty had been extracted from the coals, and thus accounting for the enormous consumption of coals per foot of grate, which must have taxed the efforts of the firemen to the utmost. To the same cause is to be attributed the vast quantity of dust and cinders blown into the tubes and smoke-box and thence up the funnel, which is one of the troubles of the forced draught system. Hence it can only be used for short spurts of a few hours, as is admitted by the Admiralty authorities. How impossible it will be to use it for merchant service on long voyages is evident.

It is remarkable that, while the forced air system results in diminishing economy as the blast is raised, and there is a maximum pressure after which it is difficult to work at all, the induced air on the contrary gives an increased draught, with no limit but the convenience of obtaining high speed of fan. The gases, being drawn through the tubes, instead of forced, are properly consumed and there is no discharge of ashes through the tubes. So complete is the combustion that to the author's surprise the quantity of coal used does not increase at anything like the proportion of the increased efficiency.

In conclusion, the author is desirous of drawing the earnest attention of engineers to the necessity of balancing the supply of air to the quantity of coals to be consumed. Without attention to this both economy and efficiency are sacrificed. If too little air is supplied the gases pass off unconsumed, and if too much is forced in the gases are driven off before they have

time to be consumed, fuel is wasted, and the temperature lowered.

The annexed tables show comparative results of natural draught on full-speed trials in the Navy, and induced draught on full-speed trial with the author's experimental boiler.

After the reading of the paper, Mr. Martin explained the mode in which the temperatures had been taken, and produced the testing rod which he had used in several war ships. Into the hollow end of this rod pieces of wire $\frac{1}{4}$ inch diameter and 1 inch long were inserted. These were of various metals whose melting points were known—tin, bismuth, lead, zinc, &c.—forming a scale. He had frequently melted brass wire (1883°) but never copper (1950°), in the furnaces. By the courtesy of the Admiralty officials, he had been present at a large number of the full speed trials, and had taken the temperatures with natural draught, but not any with the forced draught. He then showed and explained a model of the anemometer he used, which consisted of a thin disc, hung on centres from the top, and actuating an index which showed the rate in feet per minute. The apparatus had been specially designed for obtaining rapid indications.

A third model exhibited was that of a fan for the induced draught, with its fittings, mounted on the uptake of a furnace with the base of a funnel over it. The driving gear attached, which consisted of an internally grooved friction pulley actuating a grooved pinion, was arranged for a proportion of five to one, but other convenient rates could be used.

Mr. Martin then showed practically that a flame, when drawn through the boiler tube by the fan, did not impinge on the edges of the orifice, as was the case with forced draught.

DISCUSSION.

The PRESIDENT, in inviting a discussion, observed that the system of forced draught with closed stokeholds had been in use for some time past with great success in ships belonging to the Royal Navy. He had himself had opportunities of inspecting it in operation on several torpedo-boats. The system, with certain modifications, had also been introduced with good results in merchant vessels, as stated in a paper which he heard read by Mr. James Howden before the Institution of Naval Architects a short time since. He therefore thought that Mr. Martin's statement in his paper as to the want of success was not quite correct. He had before him a copy

TABLE I.

Natural Draught on Full-speed Trials.	H.M.S. *Triumph.*	H.M.S. *Grinder.*	H.M.S. *Boadicea.*	H.M.S. *Sultan.*	Marine Boiler at Portsmouth Dockyard.*
Cubic feet of air consumed per lb. of coal	192·51	167·42	264	..	370·64
Cubic feet of air consumed per square foot of fire-grate per minute	86·7	30·7	86·88	..	94·61
Lbs. of coal consumed per square foot of fire-grate per hour	27·05	10·98	19·74	..	15·28
Lbs. of water evaporated per lb. of coal, at temperature of feed 212°	..	..	..	..	10·8
Temperature in combustion chamber	1834°	1800°	1850°	1892°	..
„ „ „ tubes	..	600°	442°	736°	..
„ „ „ smoke-box	736°	700°	700°	612°	..
„ „ „ funnel	..	730°	800°	810°	..
„ of feed-water	..	..	..	..	50°

* The flue of this boiler led into the factory stack, producing furnace draught at the rate of 900 feet per minute.

TABLE II.

Induced Draught on Full Speed Trial of the Author's Experimental Boiler.	
Cubic feet of air consumed per lb. of coal	354
Cubic feet of air consumed per square foot of fire-grate per minute	130
Lbs. of coal consumed per square foot of fire-grate per hour	21·92
Lbs. of water evaporated per square foot of fire-grate per hour, at temperature of feed 212°	268
Lbs. of water evaporated per lb. of coal, at temperature of feed 212°	12·25
Temperature in combustion chamber	1800°
„ at discharge from fans	450°
„ of feed-water	54°
Mean pressure of steam in boiler	76 lbs.
Diameter of fans	24″
Revolutions of fans per minute	1150

of a paper read by Mr. Richard Sennett, R.N., at the Naval Architects' meeting, in which the results of working with forced draught in some of H.M. ships were given, and also of Mr. Howden's paper. Some months ago he inspected a full-sized working model of Mr. Martin's apparatus for producing induced draught, as fitted up at Mr. Martin's factory, and it certainly worked well and gave promise of practical success. He had since seen a small marine boiler which Mr. Martin had also had fitted up on his system at his works, and that also worked very successfully. Satisfactory as it was it did not, however, adequately represent a boiler in actual service in a steamship; although affording *prima facie* evidence of practical success.

Mr. SCHÖNHEYDER said that the subject before the Society was a very interesting and important one. Of course a forced draught had been tried for some time, and tried successfully upon a very large scale in Her Majesty's Navy. But the object of a forced draught was not always the same. It was one thing in Her Majesty's ships, and it was quite another thing in merchant vessels or on land. In war vessels, including torpedo-boats, the object in view was to get as much steam as possible out of certain boilers which did not occupy a large amount of space, irrespectively of what the cost might be. But that was not the case in the merchant service or on land. At sea they could not afford to carry very large boilers, and they could not have an excessively high funnel, and when excessive speed was required, it was of great utility to have a forced draught in order to quicken the combustion, and increase the generation of steam. There were two modes of attaining this end. One was to force the air into the furnace, with a close stokehold, the other was to draw the air out in the manner which the author proposed. What had been proposed in the paper, had, he believed, been tried before, and had been abandoned. He believed that the reasons for the abandonment were twofold. In accelerating the draught by induced currents, or rather by suction, they had two or three times the volume of air to act upon, compared with what they had when the cool air was forced in. Secondly, the fan was exposed to high temperatures in the uptake, and he had no doubt that in many boilers, especially marine boilers, where the heating surface was limited, and the temperatures in the uptake were very high, a fan would soon be destroyed. The reason that the fans were not destroyed in the experiments which Mr. Martin had made was, he supposed, that the temperatures in the uptake were not high. In spite of the protecting shield which Mr. Martin put round the shaft, he (Mr. Schön-

heyder) did not see what there was to prevent the shaft being made red hot and crippled, and the fans being destroyed. If there was any merit in the system he should be the first to acknowledge it, but was there any merit in it? He was sorry to say that he could not see that Mr. Martin had brought before them a single positive proof that he had obtained better results than would have been obtained without forced combustion in his boiler. Before bringing such a paper before the Society, Mr. Martin ought to have made two distinct experiments upon the boiler, and have stated exactly the amount of water evaporated and the consumption of coal, with and without the forced draught. New results were very valuable if measured by a proper standard, and a proper rule. Mr. Howden's paper, for example, told them that without the forced combustion so many pounds of coal were burnt per square foot of grate per hour, and with the forced combustion, the consumption of coals was, say, three times the quantity. But what had that to do with the efficiency of the boiler? Was the boiler any better on that account? He should say that such a test was measuring the result by an indiarubber rule. In the same way, the statements contained in the paper were valueless for want of a proper standard with which to measure the results. He was sorry to say that Mr. Sennett fell to a certain extent into the same error, although in his paper he gave a somewhat better standard, and measured the results by the amount of steam which was raised, showing both the quantity of extra steam and the economical effect. A locomotive was not a whit better than any other boiler because it burned so many hundred pounds of coal per square foot of grate. It was no better than a Cornish boiler which burned only 8 lbs., and in which, therefore, the combustion was slow. The measuring of the efficiency of a boiler simply by the amount of coal consumed per square foot of grate per hour, ought to be strenuously opposed.

Mr. W. H. White said he thought that the last speaker had unintentionally done an injustice to Mr. Sennett. Mr. Sennett's paper, which was read at the Institution of Naval Architects, was written with a definite object and did not purport to be a complete account of experiments. It was an account of experiments made in different ships with boilers of a particular type, of which the performance was well known, and from which it was desired to obtain a very largely increased production of steam for comparatively short periods in circumstances of a special nature, economy not being in question. Incidentally they had learned that they could get about 40 or 50 per cent., and in some cases 60 per cent. more indicated

horse-power with an increased rate of consumption per horse-power per hour of not more than 10 per cent., in boilers of a certain type. In some cases ships which would have steamed 15 knots with natural draught, had steamed 17 knots with forced draught. Those who had not had experience of closed stokeholds under forced draught trials assumed too often that they could only be used for very short times. He had himself been in ships which had run 7 or 8 hours under those conditions. In large vessels the air pressure, measured in inches of water, did not exceed $1\frac{1}{2}$ inches. In torpedo-boats it was often as much as from 4 to 6 inches, the results being of course proportionately wasteful of fuel. He had had opportunities of knowing the details of most of the systems at work in war-ships, and the conclusion that had been forced upon him was that, up to the present time, no method which had been suggested approached the closed stokehold system in usefulness and adaptability to war-ship conditions. When one passed to merchant vessels the case was entirely different; although he was not at all clear that it had as yet been established that the closed stokehold was not adaptable to merchant vessels, and he was of opinion that in short passages, as in the case of the Channel steamers, the closed stokehold would probably be found the best system even for mercantile vessels. He was ready to admit, however, that for long sea passages assisted combustion with open stokeholds would be found to be advantageous. There were cases in the Navy in which there were tortuous uptakes and the funnels were extremely ill placed on account of the existence of turrets or batteries, in which most excellent results had been obtained from what he would call "assisted combustion," without the stokeholds being closed down at all. There had been an increase of power of about 20 per cent. above that which had been obtained under similar circumstances without the fans. There was no doubt that in a merchant ship a great deal must depend upon economy of consumption. The coal bill must be kept down. It was quite possible that in such cases some other means of delivering regulated quantities of air to the furnace would be found better than working with closed stokeholds. He did not think that any system which had yet been tried had such claims upon the mercantile marine as to be certain of success. In saying that he did not wish for a moment to detract from the great credit which was due to Mr. Howden for the trials which he had carried out. He was sure that in the mercantile marine forced draught would come into use. Every thoughtful engineer with whom he had conversed for some years past was

clearly of that opinion. In 1878 his friend M. Bertin showed his system at the Paris Exhibition, and gave the results of trials in several French ships. It was only just to the French to say that they showed English engineers the way in this matter of forced draught. M. Bertin's system consisted of having an air-compressing engine entirely outside the heated portion of the uptake and delivering compressed air into the uptake by means of circumferential jets. About the same time, Herr Friedmann, an Austrian, the inventor of an injector largely used in the Navy, proposed to assist combustion by a system of steam jets arranged in the base of the funnel. This plan obtained a good deal of attention. In both these cases the apparatus was of course outside the uptake, and there was no such objection as arose from the presence of the fan in the uptake. But he did not think that either of those systems approached in steam-producing power, leaving economy out of account, what had been done with the ordinary system of closed stokeholds. Mr. Marshall's plan, which had been alluded to by the author of the paper, must, he (Mr. White) thought, be looked upon as a laboratory experiment, and he did not think that Mr. Marshall himself treated his results as those which would be obtained in actual practice. The furnace temperatures which Mr. Marshall gave were far in excess of those obtained with the pressures used in war-ships. He quite understood the author's motive in bringing forward the paper, and he had no doubt Mr. Martin, with his experience and ability, had carried out his ideas in a practical shape; but he (Mr. White) joined with the previous speaker in saying that, so far as he had been able to follow Mr. Martin's explanations, the means of forming a very exact opinion upon the merits of his proposal has not been given.

Mr. Schönheyder asked to be allowed to thank Mr. White for coming to the meeting and giving them so much information. He wished again to protest very strongly against any comparison being made between the values of two boilers simply because one burnt more coal per square foot of grate per hour than the other. How would the comparison between the boilers be made supposing that there was no grate at all, as when petroleum was burnt by being blown into the furnace.

The President concurred with Mr. Schönheyder in thanking Mr. White for his observations. He would supplement Mr. White's remarks with a few figures from Mr. Sennett's paper which would, perhaps, have a slight bearing upon what Mr. Schönheyder had said. In the trial of the *Rodney*, which

took place on the 13th of June, 1885, the proportions of boiler and engines being such that, with a natural draught alone, an indicated horse-power of from 7500 to 8000 was developed, a four hours' trial with the forced draught showed a result of 11,158 horse-power. With the *Howe*, a sister ship, a trial was made on the 14th January, 1886, and the maximum indicated horse-power developed was 12,118, with an average of 11,725 over the four hours' run, showing an increase very much in the same proportion. The *Rodney* and the *Howe* had each twelve boilers. A trial was also made with the *Mersey*, a twin-screw cruiser, having six boilers. The natural draught appeared to develop about 4000 indicated horse-power, but with the closed stokehold the power developed was 6628 horse-power. The torpedo cruiser *Scout* and other vessels were also tried, the results being practically the same as those of the *Mersey*.

Mr. MACGEORGE said that having had a great deal to do with merchant steamers, he had no doubt at all that a great economy would be effected by increased artificial draughts, whether induced or forced. He thought that the fuel which could be consumed per foot of grate was of some consequence, because, when much was consumed, the consumption would be more complete, and therefore the same result would be obtained with inferior or refuse fuel, the extra oxygen which went into the furnace to support combustion being in this way the means of leading to increased economy.

Mr. J. BERNAYS wished to call Mr. Martin's attention to one or two points. Mr. Martin had shown them a very interesting experiment with the induced draught, and he had shown that the cold air passed along the circumference of the tube, and that the hot air passed along the centre. This fact seemed to imply that the arrangement must be an uneconomical one, because the layer of cold air round the hot air would prevent the heat of the gas from being communicated to the tube; whereas he conceived that in the case of compressed air the state of things would be quite different. Twenty-five years ago he tried an experiment with a screw-propeller in a shaft at the top of the boiler. The propeller was moved by jets of steam let into it by an elaborate arrangement in the centre, so that it worked like a reaction wheel. It worked very well so far as producing a draught, but it was very uneconomical with regard to the amount of air that was passed through the shaft and the amount of steam used for doing it, and therefore the propeller was abandoned. Another point to be considered was that when they drew in air by

induction, they had always to deal with cold air, whereas compressed air could be made hot before being allowed to reach the fuel and so caused to help the combustion. With regard to the shaft of the fan, he did not see any difficulty in making it without any shaft. There might be two fans; but he agreed with Mr. Schönheyder that more data were required to enable them to judge of the merits of the proposal.

Mr. Druitt Halpin said that he saw some trials with forced draughts a few days ago with Perret's furnace. He believed that the object there was to deal with refuse which could not be dealt with in any ordinary way. They were dealing with sweepings of very fine coal and also the breeze resulting from ordinary gas coke. The financial result was to reduce the cost to 5*s.* per thousand gallons of water evaporated with the waste fuel, as compared with 10*s.* per thousand gallons evaporated with ordinary coal. The thermal results given by these tests were bad, as the efficiency was low, although the quantity of air passing through the furnace, as ascertained by experiment, was only that theoretically required to support combustion, so that no large quantity of heat was accounted for as passing away in the waste gases. The thermal efficiency of the fuel had not been ascertained.

Mr. Rigg said that he did not profess to know much about marine boilers, but speaking as a land engineer he should consider that an induced draught would be used when it could be obtained, and that a forced draught would be used only when an induced draught could not be got. If they took a land boiler as an illustration, they would naturally want to get the best efficiency they could out of the coal where the heat was the most intense and they would use the gases at the highest possible temperature, and so create the motive power which was necessary to draw the air through the tubes. After all it was a question of the power to be used for getting the air through the tubes. If they took either a forced draught or an induced draught, as used in a marine boiler, in both cases they used what they might call the best steam they had got in the place for doing the work which the low temperature air would not do. He perfectly understood that, with the low funnel which they had in marine boilers, weight could no longer be given to that consideration, and it simply became a question of forced draught of some kind or other, in order to get the highest possible efficiency out of the boilers. He thought that after what they had heard that would be a point upon which they were all agreed. The only difference of opinion

would be as to the means of doing it. There must be some reason why a forced draught was better than an induced draught, and one which it was not very easy to see at the first because the result was nearly the same in both cases. Some years ago he had to make some machinery for drying cotton for the manufacture of gun-cotton, and he designed the machinery so as to drive a blast of wind through the cotton. He showed the design to one of the most intelligent officers in Her Majesty's Service, and he said, "Why do you blow the air in? You ought to draw the air through." He (Mr. Rigg) replied that he did not see the difference. The officer replied, "There was a very great difference. If you blow air through, you blow only a certain volume, whereas if you draw the air through a very much larger quantity passes, and it has the capacity of absorbing more moisture." He (Mr. Rigg) drew the air through the cotton, and the result was that the cotton was several times set on fire. It appeared to him that in the forced draught they had the advantage of having a comparatively small quantity of air in the furnace, and the amount of heat in the air was much greater in proportion than when an induced draught was used; and therefore the gases would be much hotter when the air was blown through. This was the only reason which he could see why one method should be better than the other. There was a French boiler shown in diagram No. 2, but he fancied that the design was intended simply for publication, and was not really the boiler as constructed, for there were several features about it which he thought that the French engineers would not adopt. Mr. Martin had not given the speed at which the fans had been driven, but it seemed to him (Mr. Rigg) to be wholly unnecessary to have so much extra driving gear; in fact, the more simple machinery could be made, not only on board ships, but everywhere else, the better.

The PRESIDENT said that he had hoped that Mr. Yarrow would have been present that evening. He would, perhaps, have said something on the subject. It might, however, be interesting if he (the President) mentioned that a few years ago Mr. Yarrow gave him a hard ring, composed of agglomerated coal atoms, one of a number which had been taken from the ends of the tubes of a boiler in one of Mr. Yarrow's torpedo-boats. He believed that Mr. Yarrow was not quite clear as to how it had been produced, and he was not aware whether Mr. Yarrow had remedied the defect, but the point was an interesting one.

Mr. WHITE said that the ring was obtained after the best

Welsh coal had been used in the boiler of a torpedo-boat. Mr. Yarrow had learned how to conquer the defect by using a special kind of patent fuel.

Mr. SCHÖNHEYDER, in explanation, said that he should be exceedingly sorry to do Mr. Sennett or any one else an injustice by his remarks upon his paper. Column 7 of the table in Mr. Sennett's paper was a comparison of the value of the boilers according to the indicated horse-power which was obtained per square foot of grate; but Mr. Sennett acknowledged farther down in his paper that the grate area could only be used as a fair basis for comparison for boilers similar in design and construction. If Mr. Sennett knew that the comparison was not fair, why not leave it out altogether? In column 8, however, he gave a better comparison, namely indicated horse-power per ton of boiler. That was a much fairer comparison, but it was not altogether fair for boilers which worked at the higher pressures. If they had a column containing the indicated horse-power per square foot of heating surface in the boiler, they would have a better comparison. A standard of comparison could not be too accurate.

Mr. MARTIN, in reply, said that in reference to the heating of the fan, to which allusion had been made in the discussion, it was running every day, and he had purposely tested it. He put in a small fan at first, and when taken out he could not trace the sign of fire upon it. The present fan had been working with Welsh coal, and upon examining it recently he could not find that it had been affected in the slightest degree. Certainly the shaft was as good now as when it was first put in. It had no tendency to get overheated, for the more complete the combustion was in the furnace the cooler the funnel would be. Some time since the Admiralty placed at his disposal the steamship *Grinder* at Portsmouth for the purpose of experiments, and he experimented for a week round the Isle of Wight. He shortened the furnaces a third and admitted more air and obtained 25 per cent. more duty out of the short fire than had previously been obtained out of the longer one. The temperature which the engineers had had in the funnel was 760°. The average temperature which he had in the funnel was 450°, though he was doing a greater amount of duty. This showed that the greater the efficiency in the furnace the cooler the funnel. The fan kept so cool that a person could bear his hand upon it while it was working. He had found that he had just doubled the efficiency of the boiler as compared with what was done with the natural draught. He did not make use of

high-speed engines, but he could drive his fan faster upon this system than it could possibly be driven with a high-speed engine. One of the drawbacks of the forced air system with the high-speed engine was that it might break down with continuous running and at times it became a very dangerous thing. He could go from Liverpool to New York without having occasion to stop the fan during the voyage. As to cold air going through the tubes, it was erroneous to suppose that such a thing happened. No cold air went into any part except between the fire-bars. His object in working the model prior to the discussion was to show that the flame did not impinge upon the orifices of the tubes. He had examined the boilers of a great many ships at Portsmouth. He examined the boilers of the *Polyphemus* and directly he saw them he discovered the reason of their failure. The effect had been just the same as if they had put forced air into a locomotive. They would burn the tube-plate, and they would not be able to get a boiler of that class to stand it; and furthermore the boilers would prime so vigorously that they would force all the water into the cylinders. It would throw a great deal of light on the forced draught if some of the mishaps were mentioned as well as the successes. He had spent a great deal of time in the Government service and had seen difficulties which could not be easily got over. The locomotive boiler had not a chance with forced heat upon the tube-plates. That was one of the faults of the *Polyphemus* boilers. In the case of the *Satellite* boilers the tube-plates were put at a distance from the furnaces, and they had a chance by lowering the air pressure, but they could not be worked with certainty with a three-inch pressure.

Mr. WHITE explained that the boilers of which Mr. Martin was speaking in connection with the *Polyphemus* were the locomotive boilers, which were put in with great hopes, but which unfortunately did not succeed. The boilers of the *Polyphemus* were now of the *Satellite* type. They had been worked with the full air pressure that was tried without success upon the locomotive boilers.

Mr. MARTIN said that in the *Polyphemus* the consumption of fuel was 48 lbs. per square foot of grate per hour. That was something enormous. In the *Howe* it was only 33 lbs. for the same indicated horse-power. The cause of the difference was not explained.

Mr. WHITE said that the air pressure under which the stoke-holds of the *Polyphemus* were worked approached closely the conditions of a torpedo-boat. The boilers of the

Polyphemus were not return tube boilers, and the air pressure was very much higher than was used on larger ships.

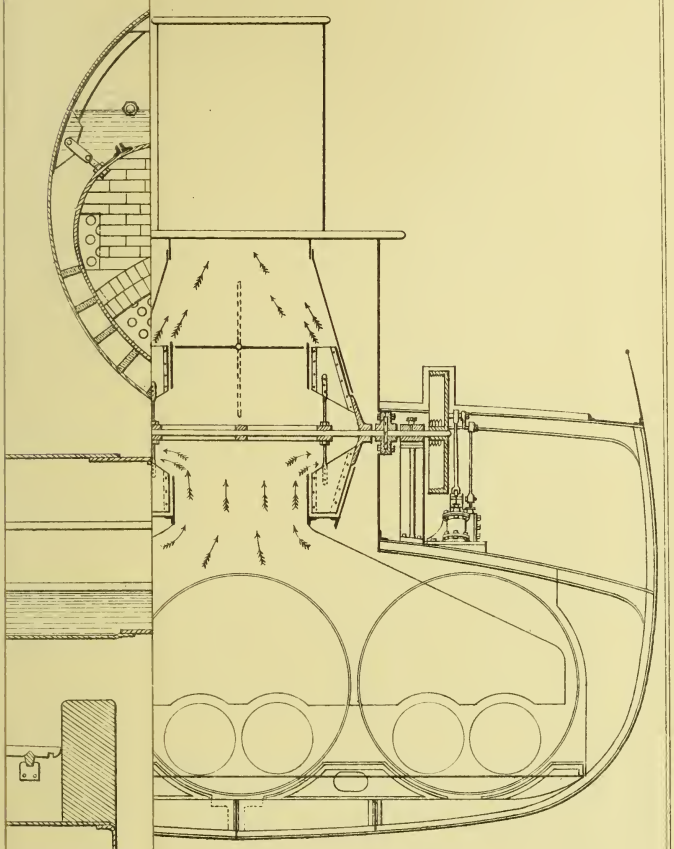
Mr. MARTIN said that he did not interfere with the stoke-hold arrangements. It would be very difficult in merchant ships to close some of the stokeholds; and the cinders and dirt which were blown up by the forced draught would be a nuisance in a passenger ship. He objected to the principle of blowing to boiler fires. The blow-pipe action localised the heat and the circulation of the water would not go on regularly. The more uniformly the heat was spread over the boiler, the better the circulation and the drier the steam. The priming of boilers often caused a great deal of trouble when a forced draught was used, and was one of the greatest dangers arising from the system, by the breakdown of the engines.

Mr. WHITE said that he did not like to interpose again; but he wished to say that his experience did not accord with Mr. Martin's. He had been present at a forced trial of seven or eight hours in a cruiser not belonging to the Navy, but one with the building of which he had to do in the North; and on no occasion did any priming take place. Priming was not necessarily an accompaniment of forced firing, as it was carried out in the Navy.

Mr. MARTIN, in continuing his reply, said that the speed of his fan was 1150, but he could increase or decrease it to suit the varying kinds of fuel. He was going to try about a dozen different sorts. Some coals would take more air than others. He had been trying to define how much air should be admitted per pound of coal. In one case he had found that 354 cubic feet of air per pound of coal gave a very good result. He had evaporated 12 lbs. of water per pound of coal. That was an economy at which he was rather surprised.

The PRESIDENT said that Mr. Martin was one of those who had a very deeply rooted antipathy to forced draught, and, as he was a gentleman of considerable experience upon the subject, he no doubt had some good solid grounds for his objection. He had endeavoured to devise something better than the forced draught, but he had not gone the whole length of the matter at present. He ought now to persevere to the end, getting an apparatus fitted up on a steam vessel; first testing the efficiency of the boiler under ordinary conditions, and then under the improved conditions of induced draught, and comparing the results. Both sides of the question had been fairly argued, and Mr. Rigg had certainly given some sound reasons why there should be preference for the forced draught as against the induced draught. Whatever conclusions might be

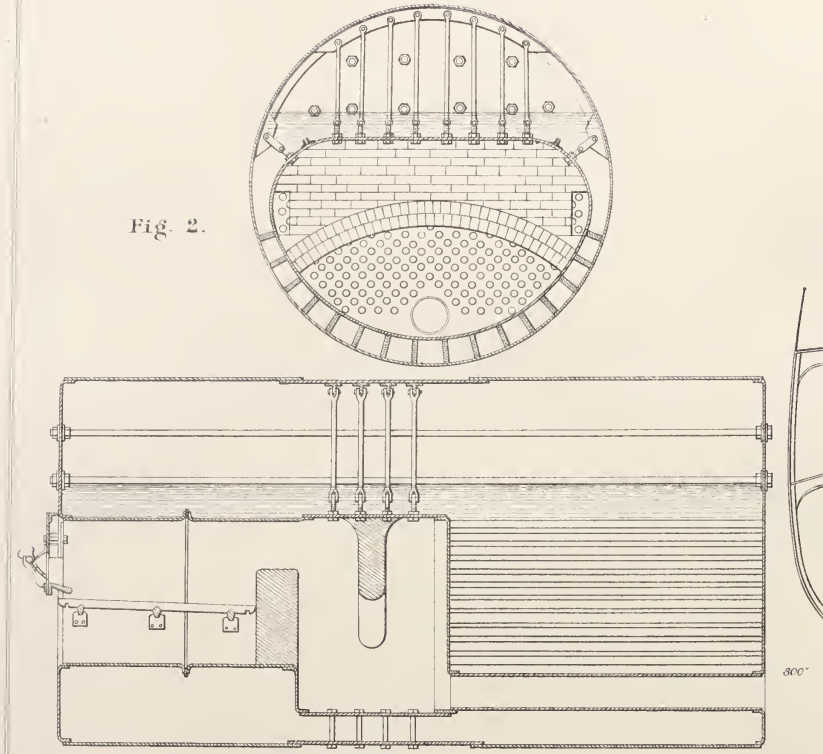
arrived at with regard to the question, there was a great deal of ingenuity in Mr. Martin's invention. The marine boiler which Mr. Martin had erected at his works had not been there a sufficiently long time, perhaps, to allow of conclusive experiments, but these should be made without further delay, so that the efficiency of the system might be definitely established or otherwise.



BOILER PROPOSED BY AUTHOR.

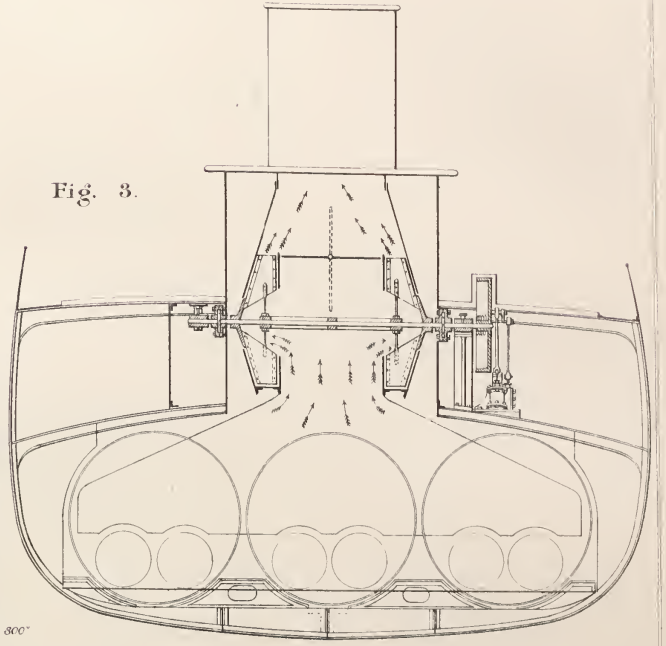
(DUCED DRAUGHT.)

Fig. 2.



BOILER FOR FRENCH GUNBOAT.
(FORCED DRAUGHT.)

Fig. 3.



SYSTEM PROPOSED BY AUTHOR.
(INDUCED DRAUGHT.)

June 7th, 1886.

PERRY F. NURSEY, President, in the Chair.

SOME MODERN IMPROVEMENTS IN THE MANUFACTURE OF COAL GAS.

By R. P. SPICE, C.E., Past President.

Having undertaken, at the request of the President, to write a paper on this subject, the author has felt some difficulty in determining what points of interest to introduce, and how to arrange the materials which lie ready to hand, and are present in the minds of all who are familiar with the subject.

Reflection soon solved this preliminary problem, and enabled him to draw the line at a memorable period in the history of coal gas manufacture, dating from the disuse of iron retorts, and the adoption of clay retorts in their stead.

The contention in favour of iron retorts, by the conservative gas-makers of that day, was perseveringly maintained on the ground of the advantages resulting from working at lower heats than would be possible with clay, the gas so produced being of higher illuminating power than when the coal was carbonised at a higher temperature.

On the other hand, the advocates of carbonisation by means of clay retorts, claimed the superior advantages of obtaining a greater yield of gas per ton of coal, resulting from high heats; and the greatly reduced cost of wear and tear of clay *versus* iron.

It is unnecessary to go at any length into the merits of the controversy, which for long was carried on between the advocates of the old or the new method, the ancient, or the modern, as to iron or clay retorts; those who are curious about ancient history may gratify their curiosity at their leisure by reading the "Annals of the Time" in the early volumes of the 'Journal of Gas Lighting.' Suffice it to say that in course of time the advantages claimed by the moderns, for clay retorts, were ultimately fully acknowledged, and the use of clay became established in every gaswork, which was carried on with any tolerable degree of intelligence, in the United Kingdom.

But in the early days of clay retorts, it was found that

the removal of the carbon, or, as the workmen called it, "the scurf," from the inside was a very serious drawback, because the operation, involving first the lowering and then the raising of the heats at which they were worked, and the consequent contraction and expansion of the material, occasioned cracks and fractures to such an extent that, for longer or shorter periods after the removal of the carbon, nearly one-half the gas evolved from the charges of coal escaped, through these often occurring defects, into the ovens, and was there consumed, instead of going forward into the gasholders.

In this state of things the inventive faculties of mechanical engineers were brought to bear on the difficulty, which they soon annihilated; the introduction of the exhauster being the result, and thus the evil proved to be a "blessing in disguise."

By means of the exhauster, which, as its name indicates, is a kind of air-pump, the gas was pumped or drawn away from the interior of the retorts, and they were thus relieved of the pressure by which the gas was prevented from flowing freely onward; this pressure being occasioned by the weight of the gasholders and the retarding action of the condensers, washers, and purifiers; amounting generally to the displacement of about 9 or 10 inches of water.

A variety of exhausters soon appeared, some engineers preferring the pumping action and others the rotary. But in course of time the steam jet was enlisted in the service, by Mr. Cleland, of Liverpool; and in the hands of the designer it was a decided success; but in many instances it proved to be a failure, and it was ignominiously turned out of some works.

This invention, as applied to gas-making, attracted the author's notice, and the interest it excited led him to devote some attention to the question of its practical utility and economic value; and having inquired into the causes of its failure to give satisfaction in several instances, he came to the conclusion that in itself it was a good thing, but that like many another good thing it depended upon being properly used, and that in introducing it the first consideration should be to determine its proper place between the retort stack and the station meter. He found, among other debatable points, that opinions were divided as to whether it should be fixed before or after the condenser, and his decision was that its proper place was between the retort stack and the condenser.

Having arrived at this conclusion, he ventured to include

the jet exhauster in his design for a new gaswork, which it was intended should be a model, embracing all the best methods and appliances then known. This was eight or nine years since, and the works were completed and brought into action in 1879, provided with two of Korting's jet exhausters, each being equal to much more than the then entire make of gas, so that one only would be required in use at a time. The make of gas at these works is now about 120 million feet per annum, and from the first starting of the works to the present time, a period of nearly seven years, one or other of these two exhausters has done all the work; one being worked for about six months, and then the other; the sole object of changing over being to see that the inlet and outlet and bye-pass valves were in perfect condition, and ready for instant use in case of need.

So perfect has been the action, so steady and noiseless, that the author has known an eminent Civil Engineer, well acquainted with gas manufacture, to pass through the exhauster room, when inspecting these works as a visitor, and on coming out, asking where the exhausters were? having no idea that all the gauges which he had seen were open to and connected with a steam-jet exhauster; the author was the showman, and, up to that point, had been silent as to the steam jet and its value.

However, he then pointed out, as he is bound to do now, the advantages appertaining to it, namely, the saving in first cost of a steam engine, which is indispensable for driving a rotary exhauster either by direct connection with spur-wheel gearing, or by shafting, pulleys, and straps; the consequent cost of the maintenance of such machinery, and the constant attendance of a skilled workman to regulate its motion, and to see that nothing goes too fast or too slow. While these jet exhausters, their regulation being automatic, will do the work, maintaining a perfectly steady gauge, without any attendance whatever from one end of a month to the other; for months together.

An objection has not unfrequently been made to the adoption of the steam jet on account of the condensation of the steam increasing inconveniently the quantity of weak ammoniacal liquor; but at the works in question no such inconvenience has ever been experienced; the steam when condensed gravitates to the underground weak-liquor tank, and is pumped up and passed through one or other of the scrubbers; none is ever allowed to be wasted by exposure to the atmosphere, and so weakened by evaporation, and there is never more than is wanted; all being manufactured into sulphate of ammonia upon the works.

The next notable improvement to which attention should be directed is the modern method of effecting carbonisation by the aid of machinery.

A learned professor has, in a recent work on gas manufacture, aptly described the operation of "carbonisation as a chemical operation of great interest and importance. During this operation (as he says) the coal vanishes, and the various products of the gas industry appear in its stead. It is a process of rearrangement of atoms, whereby the oxygen originally present in the coal, is for the most part made to enter into combination with hydrogen and yield water; whilst the remainder of the hydrogen of the coal volatilises, partly in the free state and partly in combination with carbon, nitrogen, or sulphur. The fixed residue in the retort—that which is not volatilised in the retort—is impure carbon, and goes by the name of *coke*."

There is no need to go at any length into the question which many years ago occupied the inventive faculties of ingenious minds, as to how best to effect the primary objects of carbonisation most economically; whether to deposit the coal in continuous small quantities from an overhead hopper, on to the upper horizontal surface of a revolving iron web, which should receive the supply of small coal at one end of the oven through which it would deliver it to the other in the form of coke, carbonisation having been effected in the transit through the oven; or, by the more recently proposed vertical screw, with its wide blade revolving in a retort, the coal being deposited at its upper end, and carried on the blade of the screw, carbonisation being effected as the coal descended on the revolving screw. Nor need reference be made to any other of the curiosities of design, which were intended to effect carbonisation by a continuous and rapid process of evolution. All such designs may be dismissed as the day-dreams of enthusiasts.

The history of the so-called iron stoker by which several retorts were to be charged at one operation, and as many drawn immediately afterwards, may be briefly referred to as having excited vain hopes, and afforded an additional illustration of the Darwinian doctrine of "the survival of the fittest." This culminated in the permanent success of West's more simple and effective charging and drawing apparatus; by which retorts are charged or drawn one at a time, effectively and more economically than by any other known method.

By the use of this labour-saving aid to carbonisation, the prime object of depositing the coal in a thin layer on the

bottom of the retort is secured, and a much larger surface of the sides of the retort being left exposed, the heat from thence aids materially in carbonising the coal; thereby saving time to the extent, approximately, of 25 or 30 per cent. in working off the charge. There is also the further important advantage of avoiding the extraction of a certain proportion of gas of low illuminating power; which is an evil inherent to working coal when laid in a thick mass in the retort, owing to the exterior of that mass being fully worked off before the interior has been sufficiently carbonised, and thus a mixture of poor and rich gas evolved, the illuminating power of the total quantity taken from a ton of coal being lowered thereby.

Having dealt with the fundamental process of carbonisation, and the progress made towards its present degree of perfection, the next step is reached in the ascending scale, leading from the retort to the hydraulic main; the connecting link between the two being the ascension pipe, with its terminal dip in the main.

The ascension pipe, since high heats have become the rule, has been subject to choking fits; and various devices have been tried in searching for a remedy; the last scheme being a water jacket; a kind of "cold water cure." It is difficult to imagine a greater nuisance in a retort-house than the constant choking of a large percentage of ascension pipes, occasioning recourse being had to the "jumping" process to remove the obstruction; the labour attending it being very trying to the workmen, and too painfully familiar to managers to need describing.

Without anticipating such a happy issue, this evil was speedily and effectually overcome at the works referred to, by the adoption of the "Coal-liming Process," and has never been a source of trouble since that period, which occurred more than 2½ years ago.

As to the question of dips and anti-dips, experience has led the author to believe in the dip, and to reject all the anti-dip contrivances, whether automatic or such as require raising or lowering by hand.

In his opinion the first essential is, that every retort oven should have its own separate length of hydraulic main, and the pattern he would prefer is that designed and favoured by Mr. Geo. Livesey; the top of it should have a planed surface for the seat of each dip, and every dip should have its flange and terminal end surfaced and turned true in a lathe, so that when fitted to the main, all shall be of the exact same length, and consequently have precisely the same seal or dip. These conditions being fulfilled, there will be no difficulty in maintaining,

by the steam jet, a perfectly steady gauge, absolutely level, or a pressure or vacuum of one-tenth or two-tenths of an inch throughout, just as may be desired. The old method of keeping a small river of tar in the hydraulic main, is an evil which Mr. Livesey has ably exposed and exploded.

The next matter to be referred to is, the trouble occasioned, in many works, by the formation of naphthaline. Some men say they know nothing of this cause of anxiety, and I can only suppose that those happy individuals have not suffered such inconvenience because they have not been addicted to high heats; but I have found that high heats and naphthaline are correlative, the practice of the first leading to the production of the other; and as naphthaline is a nuisance on a gaswork, it corresponds to Lord Palmerston's idea of sewage, as being "a good thing in a wrong place."

What, it may be asked, is more common than to see men from the gasworks going about a city, with the familiar apparatus for blowing the naphthaline out of stopped service pipes back into the gas main, thus wasting good light-giving matter?

It is bad enough to be afflicted with this annoying disease on the works, but it is infinitely worse that gas consumers should be subjected to the nuisance of having their supply of gas interrupted by it, and obliged to send to the gas office to obtain relief from their sufferings; and yet one is constantly hearing of the nuisance thus inflicted on long-suffering gas consumers.

If the author had not had personal experience with this disease and its remedy, he could not have referred to it here with that confidence which prompts him to place on record his settled conviction, that the remedy is both simple and certain. He is quite satisfied that the light-giving hydro-carbons of which naphthaline is composed, need not be thrown down, as they commonly are, by condensation, and lost in the tar tank; but that by proper treatment of the gas, immediately after its leaving the hydraulic main, they may be permanently incorporated with it, adding to its illuminating power, stored in the gasholder, and delivered in gaseous form at the consumer's burner.

The experience on which this conclusion is founded, was obtained at a gasworks for the proper conduct of which the author was responsible; naphthaline having appeared when high heats and four hour charges were introduced; the formation was so rapid and constant, that a powerful condenser became unworkable at the end of every ten days, the pressure having risen in that time from 9 inches to 24 inches, and of such a character was the deposit that it could only be removed by

mechanical means; this went on till it became unbearable, and was terminated by the interposition of a St. John's apparatus, placed between the retort stack and the jet exhauster; when all the trouble ceased and, instead of the condenser being thrown out of action every ten days for cleansing, it went twelve months without showing any signs of the presence of naphthaline; and that experience does not vary. Naphthaline is no longer a source of trouble or anxiety.

The secret of this success lies in the method of splitting up, almost infinitesimally, the current of gas in its passage through the boxes at the bottom of the apparatus; and its trituration there in the partially volatilised products coming over from the retorts, at a temperature favouring the transformation of some of them into a gaseous and light-giving combination.

To avoid disturbing the continuity of something approaching to chronological sequence in his narration, the author will now venture to leave the solid ground of personal experience, upon which all the previous remarks have been based, and turn to the much-debated question of generative and regenerative furnaces; which, after much struggling and varied success, are now finding their way to the front. The results which have been achieved are so much in favour of these methods of firing, that no amount of talking against them will be sufficient to prevent their being adopted, wherever circumstances will permit of their being more or less rapidly introduced. The day has gone by when some 15 or 20 per cent. of the coke made can be allowed to be wasted, since, by the application of proved ingenious devices, it can be saved and the proceeds added to revenue.

Not having had an opportunity of personally testing any of the various statements concerning either generative or regenerative furnaces by practical working, the author has for about eighteen months kept an eye on the work done by Mr. Valon at Ramsgate; and has recently arranged for the erection of some settings of the simple and efficient furnaces on his generative system; the work has been commenced, and the benches will contain 112 mouthpieces.

Besides the saving of coke, if even coke be the fuel used, this furnace is exceedingly well adapted for the economic use of tar as fuel; to aid this object Mr. Valon and his assistant, Mr. Bennett, jun., have devised a special tap, which with a jet of steam behind it, acts most efficiently.

Putting these two (the furnace and the tar tap) together, it is believed that the serious reduction in the value of tar need no longer prejudicially affect the fortunes of any gas undertaking where they can be applied.

The next and latest modern improvement in coal gas manufacture to which it is proposed to direct attention, is "the Patent Coal-liming Process," by means of which coal gas is now made practically in closed vessels, there being no difficulty in working purifiers without lime and without opening one for more than twelve months at a stretch.

This process has stood the test of every-day continuous working for a period of two years and seven months, without a hitch of any kind, and produced results such as previous experience had never developed nor led to the anticipation of.

Previous to the publication of Cooper's patent, which has been pronounced by Sir Richard Webster, Q.C., to be a perfectly valid one, nothing of the kind had been known or practised. Mixing lime with coal had been thought of and done, by here and there a gas-maker, but it had not been developed, or formulated, or reduced to a method; and for all practical purposes the bare notion had borne no fruit, but was a dead letter. Systematic coal-liming was not practised anywhere, and the clear idea, when promulgated, was rejected by gasmen of high and low degree alike all round.

After some months devoted by the inventor to seeking favour and finding none, and several abortive attempts by adverse critics to find the proper way of handling the process, it met with a fair trial at Tunbridge Wells; and there, the changes resulting from causing the retorts to deliver crude gas, containing only about 20 per cent. of the normal quantity of impurities, were noted as they arose, and the working was promptly adapted to the altered conditions. As a consequence of such careful observations and adaptation of the means to the end, success crowned patient research, and a settled home was found for the manufacture of limed coal gas, amid the sylvan scenery of that lovely district.

Not to repeat the story of its progress, for it has been told from time to time as its development required public notice, it will be enough to give a very brief statement of the summarised facts at the end of two years and seven months' continuous working, and the following is the history:—

Unlike purification by lime in the state of sulphide of calcium, and oxide of iron, the coal-liming process is definite and certain in its action; whereas the older process is uncertain and unreliable, owing to causes which are not understood, as witness the misadventures of the Newcastle and Gateshead Gas Company, and the more recent case of the Crystal Palace District Gas Company.

Both these companies have unfortunately had to submit to exposure and fines, for non-compliance with their statutory

obligations respecting sulphur compounds; the former for having had, on the 9th of January, 1885, 28·29 grains of sulphur in 100 feet of their gas, and the latter 37·37 grains in 100 feet, on the 25th and 26th of March last; and three days afterwards it contained 25·23 grains, neither company knowing the "reason why" these irregularities occurred, although on behalf of the latter company an absurd theory has been propounded to account for the failure.

This system of reducing the quantity of sulphur in coal gas, by means of sulphydrated lime, which is foul lime in a particular condition, having become established, it appears in the eyes of some men to be orthodox, sanctified by custom, and that it is heretical to attempt to supersede it; so strong indeed is their faith in the established practice, and so unreasoning is their prejudice against the better method, that if they are told the facts are against it, the sum and substance of their reply is,—"So much the worse for the facts."

It is a trite saying that "knowledge comes, but wisdom lingereth"; but although the progress of wisdom may be slow, its advance is sure; let the retarding influences be what they may.

The uncertainty of the foul lime method is proverbial, the expense considerable, and the nuisance intolerable, to those within reach of the stench occasioned; whereas it has been conclusively proved, that the reduction of the sulphur compounds by the coal-liming process is so certain that, the character of the coal being known, the results of its treatment may be predicated. The expense is converted into a profit, and the nuisance is entirely abolished.

For example, as to the certainty of the sulphur being kept within prescribed limits; prolonged experience and careful observations extending over many months, have led to the establishment of the rule, that with a given class of coal, each one-half per cent. of quicklime with its own weight of water, added to the prescribed normal quantity, will diminish the sulphur a further two grains in 100 feet; and if with the same coal the quantity of lime used be reduced by one-half per cent., there will be an increase of two grains of sulphur; and I hold this certainty of result to be invaluable to gas manufacturers.

The series of experiments by which these valuable results have been determined, were made for the author by Mr. Dougall, of the Tunbridge Wells works; for whose attention and assistance he has pleasure in acknowledging himself to be indebted.

While the experiments by which the above rule has been established were being tried, varieties of Newcastle coal were

carbonised; and notwithstanding all these interrupting and disturbing changes in the working, the average of sulphur found in the gas by the chemist appointed as gas examiner by the local authority, as reported in 'Gas and Water,' was under 14½ grains per 100 feet on the average of fifty-nine official reports, made during the experimental working which extended from January 8th, 1885, to May 26th, 1886. And for the year ending December 31st, 1884, 105 testings at the works gave an average of 12·56 grains in 100 feet.

One striking advantage derivable from, and inherent to, the coal-liming process, is the important increase of 30 per cent. in the production of ammonia; and in these times of depressed values of the residual products of gas manufacture, the directors and controllers of the industry should regard this as a welcome addition to the profits.

To render the value of this additional 30 per cent. of ammonia clear to common sense, suppose the case of a gas-work producing, on the old lines, 600 tons of sulphate of ammonia per annum for sale; and that the net profit derivable at the present depressed market value does not exceed 5*l.* per ton, the net receipt from sulphate would be 3000*l.*; but by working the coal-liming process the quantity for sale would be increased to 780 tons, and at the same rate of net profit per ton, the amount realised would be increased from 3000*l.* to 3900*l.*

Added to this, there is the saving of the present cost of purification, which by this process of carbonisation is not only abolished but reduced to something less than nothing, next to no labour being required in the purifying house; the cost for one year having been only 0·0189*d.* per 1000 feet of gas purified; and the material employed in the closed vessels goes on increasing in value, year in and year out, for how long has not yet been proved; but at Tunbridge Wells it has gone on for two years and seven months, and is going on still; the same oxide of iron having been used from the commencement. Some of it contains 90 per cent. of sulphur, and it remains, at present, a problem unsolved, as to what the percentage may be before the material will cease to carry its burden of sulphur.

There are other financial advantages which might be enumerated, but the author in this paper will only add his conviction, that the coal-liming process, whether it be considered from an economic or a sanitary point of view, is alike worthy of the highest praise which can possibly be accorded to it; and among substantial improvements in gas manufacture, it is in his opinion the most important that has been introduced and established in the present generation.

DISCUSSION.

Mr. Alfred Penny said that he had followed the paper with very great interest and pleasure. He did not think that a clearer statement of the subjects treated had ever been made, although, of course, a great deal of what the paper contained was common knowledge to gas engineers. It was, however, satisfactory sometimes to hear even a repetition of that which they all knew, when, as in this case, it was tersely and ably put. He quite agreed with Mr. Spice with regard to the jet exhauster. It had come under his observation that many people had taken it in hand, made a failure of it, and thrown it out as old iron. Mr. Spice regarded the instrument as a valuable one, and no doubt it was so if it was properly used. As to dips and anti-dips he had made many experiments with them, but he was sorry to say without satisfactory results. In his working he had adopted the course indicated by Mr. Spice, but he had not gone quite to the same extent. He had contented himself with putting his hydraulic main upon a very solid foundation of brickwork, and then taking the ovens and dealing very much in the way which Mr. Spice had indicated, that is, having a very shallow dip indeed, forgetting entirely all about the anti-dips and the no dips. He did not believe in one or the other of them. When trying an experiment he very nearly got into a serious difficulty, for the gas came down the rising pipe into the retort-house, and nearly blew the place up. The dip or seal he believed to be a valuable thing, and a great safeguard against such accidents. As to the coal-liming process, he was bound to confess that he was very incredulous about it at the beginning. He had, many years ago, seen the coal thrown down on the floor of the retort-house, and the lime put into or over it, and the two thrown together into the retort, but it seemed to him a muddling process, and produced but little result. Mr. Spice, however, appeared to have carried out the process systematically, and to have used all the necessary means and appliances that the lime might get properly incorporated with the coal, and that it should do its work in the retort itself. So far as he could see, he believed that this was the grand secret of the result which Mr. Spice had produced. He had diminished the volume of impurities in the retort, instead of leaving them to be dealt with by the purifying apparatus, the duty of which was to arrest and remove them afterwards from the gas. In fact, he dealt with them in the initial stage, so that the bulk of the impurities never passed on to the condensers and scrubbers. He (Mr. Penny) had promised himself the pleasure of ex-

plaining the process to a board of directors of which he was the adviser, in order to induce them to give it a twelve-months' trial, although he had been obdurate in the matter up to the present time. Perhaps hitherto he had come under the denomination of those people whom Mr. Spice had described as incredulous as to the merits of the process, rather than of those who treated it as being absolutely of no value. The important results obtained by the use of the coal-liming process were, in his judgment, very much dependent upon having in use the appliances described by Mr. Spice, especially the coal-breaker, and upon properly mixing the lime with the coal. It was quite clear from the statements and statistics given in the paper that very startling results had been produced by this process in Mr. Spice's hands, which other experts had now to realise in their own experiments. Personally, he felt grateful to the author for so clearly explaining a very interesting process, which might yet come to the front and be largely adopted by gas engineers.

Mr. GEORGE ANDERSON said that when he first came to London from Scotland, where he had seen nothing but clay retorts, he found iron ones still in use in London. Mr. Spice was not one of those men who would go on for ten or twenty years using a thing which had been condemned by the hard-headed fellows in the north, and he thought that he set an example to younger men in adopting new improvements, though he did not adopt them without fully studying the matter. His (Mr. Anderson's) experience did not agree with that of Mr. Spice with regard to steam-jet exhausters. When they first came out he could not get any reliable information about them, so he bought one and fixed it at his works, but afterwards discarded it. One reason was that he had to use three or four times as much fuel to raise steam to work the jet exhauster as he did when using the ordinary exhauster. Another reason was that the tar in the condenser became so thick that it had to be removed by mechanical means. He did not care much about the ammonia, because they did not utilise it; but he could easily see that the quantity of steam that was condensed amongst the liquor must have been very great, and if he had had to evaporate it in the manufacture of ammonia a great deal of fuel would have been required for that purpose, After all that Mr. Spice had said, he was not convinced that the steam-jet exhauster was as good as the mechanical exhauster.

He was surprised and pleased to hear that by means of the coal-liming process Mr. Spice had got rid of stopped ascension-pipes. He did not quite see the rationale of it, but he accepted the fact. He had had the idea all along that stopped pipes simply meant too much heat; and that all the schemes for

remedying them meant only trying to get rid of an evil which need not be created. If the gas issued from the retorts at such a temperature that the vapours of naphtha were volatilised, they got rid of all the naphtha out of the tar, and the consequence was that it remained in the pipes and became solid. This evil would be avoided by a lower temperature in the pipes. Some people had introduced larger pipes, but he held that to be a mistake, as the increased size merely delayed the result. Again, if the retorts were worked at a vacuum, and if there were any leakages, especially at the mouthpieces, they would draw in a small quantity of air, and this, mixing with the gas at a sufficient temperature to produce slight combustion, would lead to the formation of soot in the pipe. He had accidentally discovered that fact.

He had seen in the technical papers of late that much had been said about various appliances for burning tar as fuel. Mr. Spice had not said much on this subject. He had not been able to find that there was anything in the new tar-burners that was in advance of what they had known for many years. In the year 1854, while he was manager of the Surrey Gasworks, the price of tar went down from a penny to a farthing a gallon, and he set himself to devise a furnace in which he could burn the tar and in which coke could not be burned. He at last succeeded in his object. The furnace consisted of a bed-plate of tile resting on an ashpit, the plate being at an angle of 20° or 30°. The tar was run on to this inclined plane, and it trickled down and got thoroughly burned. The back end of the furnace was in connection with the ashpit, where the ash and breeze were thrown down and accumulated. When tar would not sell well, they always burned it in this furnace, and they could do so without producing a particle of smoke. He did not think that any of the schemes for introducing steam or anything else with the tar had the slightest advantage. He believed that the chief difficulty which people had found in burning tar arose from the tar not being kept sufficiently clean. He believed that tar, as made, was perfectly homogeneous, and would burn without the intermediation of filters or anything of the kind; but to keep it in that pure condition was somewhat difficult. The top of the tar tank must be covered as carefully as a money chest. For the successful burning of tar, the material ought to be kept at a temperature of 70° or 80°. At a lower temperature the tar that would heat a furnace of six or eight retorts would not be fine enough without wasting the tar; but at 80° the tar would run so fluidly that there would be no loss of it and no smoke, and the furnace, if properly made, would cause less

trouble than an ordinary coke furnace. There was not the slightest danger of burning the retort if the necessary conditions were attended to. The interior of the furnace must be constructed in such a way that the products of combustion should not be allowed to pass immediately through small channels. There must be a space over the fire in which the gases could combine before they left the retort. The question of burning the tar or selling it was simply a commercial one, and the answer depended on the relative prices of tar and coke.

He could not say that he was ever one of the doubters with regard to the coal-liming process, although, like Mr. Penny, he had never done much in it, having waited to see how the process got on. Mr. Spice had told them that he did not open the purifier more than once in twelve months. This only showed that they were arriving at a time when they would not want purifiers at all. It behoved all gas engineers to inquire into the coal-liming process. He was giving the process a trial at his works. They had not hitherto found that the purifiers lasted so much longer, as described in the paper. One thing Mr. Spice had not mentioned which was adopted at Tunbridge Wells, viz. the introduction of a small percentage of air, which would, of course, have a considerable effect in revivifying the material in the purifiers. He believed that it was Hill of Deptford who first brought that out. He (Mr. Anderson) found that a considerable advantage resulted from it during the time that the purifiers lasted, but the advantage did not approach that obtained in the working of the process at Tunbridge Wells. He wished to know from Mr. Spice the size of the box used at Tunbridge, the quantity of the material in it, and the quantity of gas that had passed through it, as these figures would enable them to judge what the process was doing.

Mr. Spice, in answer to Mr. Anderson, said that the quantity of gas made was 120,000,000 feet in the twelve months. There were six boxes, each twenty feet square. After he adopted this process, he only found it necessary to work two out of the six: previously to that he was using all of them. The regular quantity of the oxide used in a box twenty feet square was thirty tons. In the winter, before adopting the coal-liming, they had had to change one of the boxes in about ten days. At the commencement of coal-liming they took four boxes, just as they were, without any sort of preparation, and left off using the lime and worked through the oxide. One of the boxes might have been at work two or three days when they turned coal-limed gas into it, and it went three months without any air being admitted to it. Then by accident they found one day

that air had somehow gone in, because they found that only one-half the quantity of impurities left the first box, or in other words double the quantity of impurities had been suddenly arrested. Professor Wanklyn and Mr. Dougall consulted, and came to the conclusion that air must have got in somehow. They then experimented by regulating the supply of air, and found that one per cent. of air increased the effective character of the oxide in No. 1 box containing the thirty tons of oxide, while it only diminished the illuminating power of the gas two-tenths of a candle, and that was not worth considering. They had gone on in that way ever since. The air was no part of the coal-liming patent. They found that three per cent. of air reduced the illuminating power eighteen per cent. The great point of the use of one per cent. of air was that it enabled them to go on for thirteen months; and then the only reason for opening the box was that the oxide had become too much compressed, so that the box was choked. They turned all the oxide out and put half of it back again, and then they opened the box again at the end of a further twelvemonth, and again reduced the quantity of oxide by one-half. So there had been only two openings in two and a half years. There was now in each of the boxes seven and a half tons, but the gross bulk had gone on increasing by reason of ninety per cent., by weight, of sulphur having become added to the original weight of the oxide. As to the choking of ascension pipes, he found that only twenty per cent. of the impurities contained in ordinary crude gas went up the ascension pipe. Eighty per cent. was dealt with in the retort and did not go into the ascension pipe at all, hence there was no choking. That was, to his mind, a weighty argument for the process. So far from having had to buy any new oxide, they had, in fact, sold about sixty tons of it to two other gas companies. It had been originally bought from O'Neil's Oxide Company. That which they had been using had increased in value by reason of the accretion of ninety per cent. of sulphur now contained in the oxide of one of the boxes. He believed that the oxide hardened in the course of time. They were now in the third year at the Tunbridge Wells works, and he supposed that by the time that year was completed they would have to open the purifier a third time. He could not say whether the oxide got so hard that it impeded the passage of the gas, but the pressure gauge showed nothing of the kind. There had been no increase of pressure since the box was opened at the end of two years and one month.

Mr. PENNY said that he had had a box running for three months, and still giving good results; but he had had to remove the oxide, because it got so hard that the gas would not pass.

The PRESIDENT observed that he had recently had brought under his notice an invention by a Mr. Hawkins, for purifying coal gas, and which consisted in admitting about three per cent. of air to the oxide purifier. The advantages claimed for it were, increased make, great saving in purification, greater cleanliness, and consequently the abatement of a nuisance. Mr. Mitchell, the manager of the Hornsey gasworks, had visited the Salford Corporation works and the Wigan Corporation works, where the invention had been applied, and had reported satisfactorily upon it. If either Mr. Mitchell or any other gentleman who had inspected or was working the process was present, the meeting would be glad to have any further particulars that could be given.

Mr. GEORGE GARNETT said he held that purification was in its infancy at the present time. For some considerable time he had been in the habit of using oxide purifiers at the Ryde gasworks, and sometimes allowing them to run for twelve or eighteen months; and at the end of that time they had been thrown out of action simply through a certain amount of pressure having been caused. The material had hardened through being swollen by the absorption of the sulphur. The cost of the purification had been very small, but as he had not his notes with him he would not state what it was, for the sum would seem incredibly small. The result had been attained in a different way from the coal-liming process. He held it to be a cardinal point to have a very slow condensation. He held it to be a cardinal point in the next place to arrest the tar mechanically and to wash the gas, and not to be afraid of pressure. He cared nothing about twelve or fifteen inches pressure in his washer, so long as he had a good boiler and engine and exhauster behind. But the main point was washing the gas, in the first place, in the strongest liquor they had upon the works, then washing it again in one of less strength, and so going on until they washed it at last with clean water. He had no hesitation in declaring that, after nineteen months' working of one purifier, not one particle of tar had ever been found on the lower grid, or, he ought to say, the only grid, for he used it 4 feet 6 inches thick. Nor had they found the slightest indication of the pressure being affected by a hardening of the material. If he had obtained such results without the coal-liming process, perhaps some one would go to work and get higher results by the use of that process. He was an old chemist, and he believed in the coal-liming to some extent. A thin layer of oxide had been spoken of, but his experience was that the thicker the layer of oxide the better; and therefore in his purifier he had over forty tons of oxide of iron.

He congratulated his old friend Mr. Spice on what had been achieved, but he believed that the present results were as nothing compared with what would be done in the future.

Mr. C. GANDON said he had not been present during the reading of the whole of the paper, but with regard to working without seals in the hydraulic main, he might say that he was at one time enthusiastic about it, not only on account of the pressure caused by a short dip into the liquid in the hydraulic main, which he thought produced unfavourable results in gas-making, but also because when the pipes became sealed in tar, great oscillation was caused by the gas having to pass through the thick, heavy fluid. He had tried working without dips and had met with very fair success. In the Crystal Palace works at Sydenham they had one house containing eighteen benches of through retorts, each with fourteen mouthpieces, working entirely without dips, and they worked very well. But after having carefully observed them, he had come to the conclusion that the game was not worth the candle. It cost a great deal of money to put in the anti-dips, and they required a great deal of looking after, as they were liable to cause accidents through explosions and so forth. He had gone back, in a new retort-house, to the old plan of dips. He thought that it was quite right that all gas engineers should make such experiments and endeavour to introduce improvements, but it was no good to continue their experiments when they became convinced of the disadvantage of the new method. He had listened with very great pleasure to the remarks made by Mr. Anderson with regard to tar furnaces. Perhaps no one had had more experience and met with more success in that respect than Mr. Anderson. He (Mr. Gandon) had not been unsuccessful, but he must confess that he would never burn tar if he could sell it and burn coke. When they had to rely upon stokers they were very likely to get unfavourable results. Either the retorts were burnt to pieces, or the neighbourhood poisoned with smoke, or the heats were allowed to go down too low. With the present state of the tar trade they must, however, persevere with burning tar, or they might be unable to find a sale for it. He had not had any experience of the coal-liming process, but the idea occurred to him while he was abroad some years ago, and he tried the mixing of lime with coal. There was no question that this arrested a large amount of impurities which otherwise would have passed forward to be dealt with by the washers, scrubbers, and purifiers, but the coke came out in such a state that he could neither use it nor sell it satisfactorily. It burnt the furnaces all to pieces. Apart

from this disadvantage, he thought that the process was a very good one. As to the sulphur compounds, he believed that he had heard in the closing remarks of the paper that they had been brought down at Tunbridge Wells to an average of twelve grains per hundred feet by the coal-liming process. He should like to know the maximum and minimum, because if the maximum was over a certain amount a Gas Company would be liable to a fine, however low their minimum might be. There was no doubt in his mind that the coal-liming process gave a great advantage. Very possibly one reason for the success at Tunbridge Wells was that they there used a mechanical stoker which necessitated the coal being reduced to powder before it was used. By that means the coal and lime became intimately mixed, and the small amount of lime which was needed was not observable in the coke. He believed that thorough incorporation was necessary for success.

Mr. W. Sugg said that he had been very much struck with the remarkable results which were mentioned by Mr. Spice. The questions asked by Mr. Anderson showed that he thought that there must be something extraordinary in the case. Mr. Garnett seemed to have arrived at somewhat the same result. Mr. Broadberry had suggested to him (Mr. Sugg) whether there was not a building up of the sulphur in the oxide of the purifier. Pure lime was of no use at all for taking up bisulphide of carbon, but if the lime was sulphuretted in the first instance, it would absorb bisulphide of carbon. That was an instance of sulphur absorbing sulphur. It had occurred to him and Mr. Broadberry that Mr. Spice might have found the same effect produced in the oxide purifier. The results which had been attained by Mr. Spice seemed very great ones, especially considering the comparatively small quantity of oxide which had been used. It would seem that there must be some such action as he (Mr. Sugg) had mentioned, to cause the purifiers to take up so large a quantity of sulphur. It was, however, at present not possible to obtain any exact data in this matter. They knew that, however well the purifiers worked in taking up the sulphur impurities, unless there were several sets of purifying apparatus, there would be sure to come a time when the purifiers would no longer act upon those peculiar sulphur compounds generally known as "fixed sulphur," and there might be more of this kind of sulphur found at the outlet of the purifiers than there was in the gas which went into them. It had been found even in the oxide purifier that this "irremovable sulphur" sometimes increased through the action of meteorological changes on the oxide in the purifier.

Mr. Broadberry said that he had understood Mr. Spice to

state in his paper that he obtained ninety per cent. of sulphur in the oxide. In that case there would be only about ten per cent. of oxide. It was well known to gasmen that when oxide of iron was charged with anything like thirty per cent. of sulphur it would absorb bisulphide of carbon; and there was no doubt in his mind that by using oxide heavily charged with sulphur they could reduce the sulphur compounds to something like 10 or 12 grains per hundred feet. The effect seemed to be due to the affinity of sulphur for sulphur compounds. Mr. Spice said that he did not know to what extent this might go on, and that every time after opening, his purifier went a little longer than before. He first took out half of the oxide, and the purifier acted better; and he then took out more, and it acted better still. If the sulphur compounds in the gas were reduced to 10 or 12 grains with a purifier containing ninety per cent. of sulphur, it was a question of what all sulphur would do. Perhaps Mr. Spice would state his opinion whether it was the sulphur which enabled the purifier to run so long. As to the coal-liming process, he worked very hard at that process twenty years ago, using about double the quantity of lime which Mr. Spice was using; but, like Mr. Gandon, he discontinued the process because he found that it spoiled the coke, and the excess of lime in the coke fluxed the furnace. There was no doubt that the process purified the gas.

Mr. Penny said that he did not clearly understand Mr. Spice whether the ninety per cent. of sulphur was *plus* the weight of the other material, namely, the oxide in the purifier; or whether the sulphur amounted to ninety per cent. of the whole material in the purifier.

Mr. Spice said that the ninety per cent. meant ninety per cent. of sulphur contained in the oxide after two and half years' action in the purifier. He had merely reduced the quantity so as to have plenty of room to work in. The quantity of oxide which Mr. Garnett spoke of called to his mind his early experience in coal-liming, when they were working with five boxes as a rule and sometimes six, on the old plan. They found by experience at that time that they had so much oxide in use that the illuminating power was a candle less when they were working with five boxes than when the number was reduced to three. The gas was improved half a candle by shutting off one box, and another half candle by shutting out a second box. The first box took off ninety-eight per cent. of the impurities; only two per cent. being left for the second box to take up. He believed that at last they got to two purifiers; but at any rate the excess of oxide of iron for purifying was clearly an evil. Anybody could try the experiment for himself.

Mr. PENNY asked whether Mr. Spice found any carbonic oxide.

Mr. SPICE said that they had about one per cent. of carbonic acid, and it cost more to take it out than the result was worth. With regard to the remarks by Mr. Sugg and Mr. Broadberry, he was not sure that it was the sulphur deposited which did the trick. There was next to nothing in the way of arresting the sulphur compounds done in the purifiers. That was done under the influence of the lime in the hot retort. That was where the secret rested, and nowhere else. It was a fact beyond all dispute that in their early experience they did for a time get remarkable results; but they had never got them since, and how they got them at first he was not prepared to say. Mr. Wanklyn and Mr. Dougall had regularly between seven and eleven grains in the hundred feet. They had never got up to twenty grains, except designedly for experimental purposes. When they had got some coal containing ash and sulphur in great and unusual proportions, he still adhered to the regulation standard of 2½ per cent. in ordinary working. There was an entire absence of "jumping up" with regard to sulphur compounds. He was just as able to determine beforehand what the results would be, as Mr. Sugg would be able to tell how much gas per hour a certain burner would consume. If they had very sulphurous coal, the rule was that the addition of ½ per cent. of lime would reduce the sulphur compounds two grains in a hundred feet. That was a proof of his statement that the trick was done in the retort. If they increased the quantity another ½ per cent., they diminished the sulphur compound another two grains. He had carried that up to 4½ per cent. of lime. He held it to be impracticable to go beyond that. The coke produced at Tunbridge Wells sold freely, and carts were waiting to take it away as soon as it was ready for sale. That was the best answer which he could give as to the effect of the process on the coke. There was no necessity to crush the coal to *powder* in the coal-liming process. Their complaint as gasmen was that in the case of Newcastle coal they got too much small and too little large. Therefore, as a rule, he did not regard it as necessary for the success of the process to break up the coal, although it would be certainly proper and better for the gasworks to possess a coal-breaker. It would not be a very great hardship to provide one; and if that was all that stood in the way of an addition of 500*l.* a year to the dividend, the sooner that all gas companies had a coal-breaker the better.

Mr. F. G. DEXTER said the question had been raised as to

the affinity that sulphur might have for sulphur. It was stated that ordinary lime was of no use for the removal of bisulphide of carbon and other sulpho-carbon compounds, but that such impurities were removed by sulphide of calcium, which was formed by the combination of sulphuretted hydrogen with the lime. He had always understood that this was the case; but as calcium sulphide was a chemical compound entirely distinct in its properties from its constituents—sulphur and calcium, he did not see that the purifying action could be assumed to be an affinity of sulphur for sulphur, and that, therefore, sulphur alone would effect purification.

Mr. SPICE said that that was a question for the chemist. He did not know what the chemical change might be. He was not prepared to speak of the chemical transformations in detail. He only knew that they had only one-fifth of the whole coming forward from limed coal.

Mr. SPICE, in further reply, referred to Mr. Gandon's inquiry as to the maximum and minimum of sulphur compounds left in the gas. The maximum had been worked up for experimental purposes on one occasion to 21 grains in the hundred feet, because he would not allow ½ per cent. of lime to be added to the quantity which was then being used. They then brought the maximum down steadily two grains at a time for every half per cent. of lime added. Therefore they might make the maximum what they liked. The whole thing was entirely under control. If they found a fine staring them in the face upon their getting up to 15 grains, then they would not get up to 15 grains. He spoke from the result of two and a half years' working, and the experiments upon which his statement was founded had been made most carefully by Mr. Dougall. There was no uncertainty about the result. If the stokers did not do their duty the tests would immediately show the fact, and a defect could be readily corrected. During one year's working of the system there was only a variation of ¼ per cent. between the estimated or supposed quantity of lime required and the quantity actually used. The average of the whole working was about 12 grains of sulphur compounds. The maximum in that period was about 15 grains, and the minimum about 6. Experiments had been made for his information with a view to ascertaining whether the proportion between the lime and the sulphur compounds in the gas could be reduced to a positive law, and it was a great satisfaction to him to find the proportion which he had stated. That proportion had been proved over and over again.

In answer to Mr. Anderson and Mr. Penny,

Mr. SPICE said that the oxide was taken out just as it was, and half was put back again. He thought that Mr. Broadberry had not followed that operation.

Mr. ANDERSON asked what Mr. Spice would state the proportion to be if he (Mr. Anderson) came to him as a buyer of spent oxide and said, "What proportion of sulphur will you guarantee?"

Mr. SPICE said that he should say, "I am selling you ninety per cent. of sulphur and ten per cent. of something else."

Mr. PENNY said that they put in 40 tons, and got out nearly 80 tons, that quantity being 40 tons of oxide *plus* the difference in sulphur.

The PRESIDENT, in closing the discussion, congratulated the Society on having had an excellent paper, and Mr. Spice on having evoked a good discussion. In November 1884 a paper was read before the Society on the Cooper Coal-liming Process, which had been again brought prominently before them that evening. During the discussion upon that paper he had expressed an opinion favourable to the process, so far as he could do so from the observations which he had been able to make at the works at Tunbridge Wells. He had previously inspected those works under the guidance of Mr. Spice and Professor Wanklyn. He had spoken then, as he would speak now, not as a gas expert, but simply as an engineer and a scientific observer; and he desired to say now, as he had said on the former occasion, that he believed that there was a future for the process. The observations he made in 1884 had, to his mind, been strengthened during the present discussion by what had fallen from Mr. Penny and Mr. Anderson. Those gentlemen were evidently becoming converts to the coal-liming process, after having held out for some years, if not as opponents, at any rate as quiet dissentient observers of the course of events. Mr. Gandon had referred to the process of mixing lime with coal. He (the President) wished to point out to Mr. Gandon that there was a wide difference between that Cooper coal-liming process and the mere mixing of lime with coal. Those two substances might be mixed in any off-hand and injudicious manner and with bad results, but he thought that, if the coal-liming process was carried out in its entirety and with care, as he saw it carried out at Tunbridge Wells, it could not be attended with anything but the success it had there.

THE VACATION VISITS.

Four visits were made during the summer of 1886, to works of interest in different departments of Engineering. Two of these were to places that had been visited the year before, the importance of the work in each case making it not only desirable, but specially instructive and interesting to compare the later with the earlier stage of progress.

The first visit was on the 30th of June to the Tilbury Docks, the latest development of dock construction for the port of London. As this important work was inspected while in course of construction, in June of 1885, the general description given in the 'Transactions' for that year need not be repeated. Suffice it to recapitulate the main outline, that the docks have been constructed by the East and West India Dock Company, to provide deep-water accommodation for steamers of the largest class, the need for which has been making itself more felt year by year. The tidal basin has a water area of 19¼ acres with a depth of 26 feet at low water of spring tides. The main dock has a water area of 23 acres, and has three branch docks projecting from it, adding 30 more acres, the bottom throughout being 7 feet above that of the tidal basin. The lock leading into the main dock is 80 feet wide, and 700 feet long. The engineers are Mr. A. Manning (who accompanied the visitors in their inspection) and Mr. D. S. Baynes, and the works have been completed, from a comparatively early stage, by Messrs. Lucas & Aird as contractors, Mr. W. Colson being their engineer and agent.

At the time of this second visit the docks were complete and ready for the admission of traffic. The fresh points of interest were the arrangements for electric lighting, the hydraulic machinery, and the steam pumps for emptying the graving docks.

The chief electric station, for lighting the docks generally, is at the northern end, and the apparatus comprises ten Crompton dynamos, two for supplying the Swan-Edison glow-lamps, of which there are 1200 of 16 candle-power each; and eight for working the 80 arc-lights of 2000 candle power each. The hotel and southern end of the docks are lighted from a smaller station near the entrance, in which are three Crompton dynamos.

The engine power at the northern station consists of two Fowler's compound engines of the "Yorkshire" type, each of 50 horse-power nominal, taking steam from two Lancashire boilers; and at the southern station, of one of Marshall's under-type engines.

The pumping engines for emptying the graving docks are four in number, driving directly a similar number of powerful centrifugal pumps, the whole being by Messrs. Simpson & Co. Jay's valve gear is used for regulating the cut-off of the engines.

The combined duty of the four pumps is 36,000 tons of water per hour, and they can either be applied to both graving docks at the same time, when the emptying is accomplished in one hour, or the whole power concentrated on one dock, emptying it in half an hour.

The hydraulic machinery is at the northern end of the docks, and consists of three pairs of compound pumping engines of 100 horse-power each, steam being supplied from three Lancashire boilers working at 80 lbs. pressure. There are two accumulators loaded to a pressure of 700 lbs. per square inch. The hydraulic machinery is by Messrs. Sir W. Armstrong & Co.

After inspecting the machinery, the members and friends embarked on board the company's dock steamer, *Hornet*, at the northern end of the central branch dock, and were conveyed through that dock, the main dock, the lock and the tidal basin, out into the river to examine the entrance works and jetties. The Hunter & English floating derrick was also an object of considerable interest. It is capable of lifting 80 tons, and the steam engines on board can also be used to drive a small screw propeller, thus making the whole structure self-contained and independent. Perhaps the chief novelty in this powerful derrick is the use of wire rope in place of chain for the lifting gear.

The second visit was, on the 10th of August, to the Water Works of the Southwark and Vauxhall, and the Grand Junction Companies at Hampton, the special object of the visit being the pair of new pumping engines at the former works, designed by Mr. J. W. Restler, the company's engineer, and constructed by Messrs. Moreland & Sons, of London; but the entire plant was inspected with great interest. The old works contain three Bull engines, which are exclusively used for pumping unfiltered water to Battersea, through a 36-inch main over a stand pipe 140 feet high, for filtration and further distribution. The diameter of cylinder in engines Nos. 1 and 2 is 66 inches, and the stroke 10 feet, the pump plunger being 39 inches in

diameter, pumping 516 gallons per stroke, with an average speed of 6·6 strokes per minute. The No. 3 engine has a cylinder 70 inches in diameter by 10 feet stroke, the diameter of pump plunger being 42 inches. The discharge is 600 gallons per stroke, with an average speed of 7·25 strokes per minute. Steam is supplied at 40 lb. pressure by thirteen Cornish boilers 28 feet long, 5 feet 9 inches diameter, with flues 3 feet 9 inches in diameter. Four of these boilers have been recently fitted with Vicar's mechanical stoker. At the new works, the pumping power consists of two Cornish beam engines, built by Harvey & Co., of Hayle, having cylinders 80 inches in diameter by 10 feet stroke. The pumps are $24\frac{3}{4}$ inches in diameter, double-acting, each delivering 371 gallons per stroke to a height of 230 feet, at an average speed of 8·5 strokes per minute. They pump direct into a 30-inch main, which supplies the reservoirs at Nunhead and some of the high districts on its route. Steam is supplied at 40 lb. pressure by 11 boilers of the Cornish type, 28 feet long by 5 feet 10 inches in diameter, with flues 3 feet 6 inches in diameter.

The new engines are intended to pump into the same main as the last named. They are of the inverted cylinder direct-acting rotative type, compound and surface-condensing; each pair has a high-pressure cylinder 32 inches diameter, and a low-pressure $52\frac{5}{8}$ inches diameter, both having a stroke of 84 inches. They are placed side by side and work on two cranks at right angles to each other. The crank shaft is 16 inches in diameter, with centres of bearings 6 feet apart; bearings 14 inches diameter by 21 inches long; crank pins 9 inches diameter by 9 inches long. The connecting rod is 14 feet long by $7\frac{1}{2}$ inches diameter at centre, tapering to $5\frac{3}{4}$ inches at ends. The fly-wheel is 21 feet diameter with a rim 18 inches wide by 15 inches on face. There are 553 tubes in the condenser, each 1 inch outside diameter; the total tube surface being 1013 square feet. The pumps are 19 inches diameter by 84 inches stroke, double-acting, placed one under each cylinder. The valves are of gun-metal, 20 inches diameter and of the four-beat type. The aggregate engine power is 976 horse-power indicated, and the duty equals $12\frac{1}{2}$ million gallons of water lifted 280 feet in 24 hours. Steam is supplied from six steel Lancashire boilers, 28 feet long and 7 feet 6 inches diameter. The engine-house itself is a handsome specimen of design.

In order to facilitate the filtration of the water, extensive works have been completed with a view of utilising the ballast stratum as a natural filter, and thus the construction of large

subsiding reservoirs has been rendered unnecessary. This renders available the large quantity of pure underground water found adjacent to the Thames at this spot. The water is raised from the bottom of the ballast stratum to a height of about 15 feet by three 3-throw pumps, each 3 feet in diameter by 50-inch stroke, driven by an inverted-cylinder compound condensing engine, manufactured by the Savile Street Foundry Company, Sheffield, and having cylinders 17 inches and 32 inches diameter by 2 feet stroke; the pumps running at a fourth the speed of the engines, the power being communicated through helical toothed gearing. These engines are at present indicating 50 horse-power, but they are capable of exerting 200. There are two subsiding reservoirs connected with the old works, having an area of about 2 acres, and three filter beds at the new works, of a total area of $3\frac{1}{4}$ acres, the filtering medium consisting of 36 inches of Harwich sand, 12 inches of hoggin, 9 inches of fine gravel, and 9 inches of course gravel, total thickness 5 feet 6 inches.

The Grand Junction Waterworks at Hampton comprise:—(1) The original works for pumping the water from the river in an unfiltered condition along a line of pipes 33 inches in diameter and $7\frac{1}{2}$ miles in length to the works at Kew Bridge, where it is filtered and distributed to the town district; (2) the more recently constructed reservoirs, filters, and engines which supply the low-level district from Sunbury to Notting Hill by an independent line of 30-inch main; and (3) the underground works for filtering the river water through the natural beds of gravel and sand, and pumping it into the reservoirs, these arrangements being brought into use in periods of flood in the river. The original works were constructed by Messrs. Aird & Sons in 1853, the machinery being supplied by Messrs. Harvey & Co., of Hayle, and comprise two deposit reservoirs, three direct-acting Cornish pumping engines, with an aggregate horse-power of 784, lifting the water over a stand-pipe, with thirteen single-flued Cornish boilers. These engines are capable of raising 14 million gallons of water per day. The new works were constructed in 1879–1880 by Mr. William Webster, and consist of a storage reservoir with 10 acres of water-surface and a capacity of 45 million gallons, three filters, and a covered filtered water reservoir of concrete. An engine-house containing two compound rotative beam engines, 150 horse-power each, by Messrs. James Watt & Co., of Soho, Birmingham, supplied with steam from the boilers before mentioned. These engines, under the terms of the contract, were to be capable of raising 8 million gallons of water to a height of 150 feet in twenty-four hours, but not more than 7 millions have been re-

quired up to the present time, about 5½ millions being the usual quantity. They have been at work for nearly four years without any repairs of importance. After they had been at work for six months, a careful trial of the duty of the engines was made by Mr. E. A. Cowper, when they were found to work at a consumption of rather less than 2½ lb. of the best Welsh coal per actual horse-power per hour. The whole of the foundations of the engines and the pump-wells are of Portland cement concrete. For the cleansing of the filters at these works mechanical aid has been called in so as to reduce manual labour to a minimum. After the surface of the sand has been scraped, and the dirty sand placed in heaps, lines of 24-inch gauge light railway are laid on the surface, on which trollies are run, having movable skips on them. These are filled with the dirty sand, and run to the side of the filter. A travelling steam crane then lifts the skips to the surface, and runs them to the sand washing boxes, where the sand is tilted out, the crane returning for another load. By this means a filter can be cleaned and renewed in a very short time, and at a small expenditure of manual labour.

The system of passing large quantities of water through natural beds of gravel and sand was first introduced at Hampton by Mr. Fraser, the engineer to the Grand Junction Company, more than three years ago, and has since then been in successful operation. In times of disturbance from floods, &c., the river water is received into an intake, from which it flows through two vertical screens of gravel of different sizes, and by 24-inch earthenware pipes into a bed of gravel and sand of great thickness underlying nearly the whole of the Hampton station, constituting a natural filter of the most efficient kind, the result of which is at all times to deliver a pure and brilliant water, the lower intake during the same period being absolutely closed. The collecting pipes, also 24 inches in diameter, with open joints, are placed about 8 feet below the summer level of the water in the river, and receive it after having gravitated through the beds, and thence convey it to the pump well of a small engine-house, whence it is pumped into the storage reservoir. The engines are two horizontal ones supplied with steam from three boilers, the whole manufactured by Messrs. James Watt & Co. These lift the water by means of 3-throw pumps into the reservoir, where it is stored and made use of in times of flood. As much as 12 million gallons of water have been treated in this way in one day. Independently of the process of natural filtration, the Grand Junction Company have at the Hampton station three filter beds with an area of 2¼ acres, through which the water intended for the low-level district

from Hampton to Notting Hill is passed before being pumped into the mains and service pipes; the remaining and more important portion of the supply being pumped on to Kew Bridge station, there to be again filtered and afterwards distributed through the metropolitan district of the company. The other important works of the company at Hampton, comprise two subsidiary reservoirs containing 6 million gallons of water, a large storage reservoir, containing 45 million gallons, and the covered reservoir, to hold the water after filtration, 2½ million gallons. The members had the benefit of the presence and guidance of Mr. Restler and Mr. Fraser in viewing their respective works.

The third visit was on the 7th September, to Kensington, where the Society inspected two places of great interest, the National Agricultural Hall, and the neighbouring electrical works of Messrs. Woodhouse & Rawson.

The National Agricultural Hall was the original idea of the late Major Burnaby, M.P. At his death the work was delayed for some time, but was, at the time of the visit, rapidly approaching completion, and the building is now open to the public. The site is on the western side of Addison Road Railway Station, and covers a space of 6¼ acres. A carriage entrance from the main Hammersmith Road, a railway entrance on the east side of the building, and a goods entrance at the north-west corner of the building, are available, all being independent of one another. The late Mr. Henry E. Coe prepared the original design of the building, as well as the fine elevation on the east side, and instructed Mr. Max am Ende and Mr. A. T. Walmisley, of Westminster, to design a roof which should best meet the requirements of the situation. At Mr. Coe's death, Mr. James Edmeston was appointed architect to the National Agricultural Hall Company, Mr. am Ende with Mr. Walmisley remaining as engineers for the iron roof over the main building, which is 440 feet long by 250 feet wide.

The structure consists of semi-circular arched ribs 7 feet deep and 170 feet clear span, having a clear height from the floor to the crown of the roof of 100 feet. These ribs are placed 34 feet apart, and are connected by main purlins fixed radially about 18 feet 6 inches apart, which, being continuous from end to end, serve to communicate a portion of the wind pressure from one end screen to the other. The distance between the screens is 374 feet, divided into eleven bays by the main ribs. In each of these bays there are light intermediate transverse ribs dividing the space into 17 feet centres, and secondary purlins run longitudinally about 6 feet 2 inches apart. The intermediate main ribs in the centre of

the 34-foot bays are doubled in width under the ridge to support the ventilator, which is 36 feet 6 inches wide. The top of the ventilator is 11 feet 6 inches higher than the top ridge of St. Pancras Station. The end screens are of novel construction, and consist of light lattice girders 4 feet wide, suspended vertically in a form resembling a vertical ridge and furrow roof, and present the nearest approach to the appearance of a folded curtain that can be produced in an iron screen. The usual heavy horizontal members are thus entirely avoided. The arched space, 374 feet by 170 feet, covered by the main roof and enclosed at ends by these screens is surrounded by a gallery 40 feet wide upon the north, west, and south sides, and 26 feet wide on the east side.

The first impression that the roof gives to the trained eye is that of exceeding lightness, but this has been attained, not by sacrifice of safety, but by most careful study of all the possible stresses from equal and unequal loading, from wind in varying directions, and by adaptation of every detail to meet these stresses without waste of material.

Although the soffit of each main rib is semi-circular from about 15 feet above the ground, the rib itself in its full depth springs from a higher level, and is therefore segmental, dividing below its springing into a vertical bracket, the soffit of which completes the semi-circle, and an inclined girder which carries the roofing of the side span over the gallery running round the hall. By this device the vertical component of the roof stresses is carried down to the columns supporting the roof and the face of the galleries, and the diagonal component is carried to the side of the building; where, however, it is not communicated to the brickwork, but taken up by a system of bracing which finally conveys it to an open-webbed girder, the base of which is 12 feet under ground. This girder, and the bracing leading down to it, are so designed that the thrust (or diagonal component of the stresses in the rib) ultimately reaches the foot of the inner column, as an upward pressure, which is there neutralised by the whole weight of the structure resting on that column, thus completing a self-contained system of ironwork, the base of which with a purely vertical pressure is the under flange of the girder above mentioned. The idea of taking up in the ironwork itself the diagonal thrust of the roof had been adopted before in the roof of the Dublin Exhibition and some others, but never with such completeness of design as in the present structure. Another point, entirely novel in a building, and intimately connected with the above, is that the columns supporting the vertical pressure of the roof ribs are pivoted at both ends, having a true ball-and-socket bearing, so

that, however unequally distributed may be the loads coming upon them, the pressure shall always be in the true centre line of the column.

At the time of the visit, the framework of the galleries was complete; nine main ribs with their connecting purlins and intermediate ribs were also finished. The Members, who were conducted over the works by the engineers, Mr. Ewing Matheson being present on behalf of Messrs. Handyside, had the opportunity of inspecting the travelling stage, which contained 10,000 cubic feet of timber, having a most commodious staircase and a top platform 62 feet wide. The main ribs being 34 feet apart, a complete bay can be fixed, with plenty of space for working, the width allowing of an overhang of 14 feet on each side of the centre of the main ribs. The upper portion of the roof was erected by sheerlegs, with the help of a crane fixed on the top of the travelling stage. The whole roof is covered with glass and zinc upon Mr. T. W. Helliwell's systems, which obviate the use of putty and painting to the sash-bars. Messrs. Andrew Handyside & Co., of Derby, were the contractors for the manufacture and erection of the ironwork, Messrs. Lucas & Son, of Kensington, being the general contractors for the building operations. In addition to the main hall there is a good-sized minor hall, with stables and other buildings, which adjoin the large hall.

The many points of novelty in construction were highly appreciated by the visitors, especially the lightness of the structure as compared with its great span, whilst the accuracy of construction was well worthy of the praise it received. It was conceded on all hands that the roof was a decided departure from ordinary practice, and was a bold, if not a daring piece of construction, and one for which the engineers deserve every credit.

Messrs. Woodhouse & Rawson's works, in the Hammersmith Road, were then visited, where the manufacture of a variety of electrical lights and fittings was seen. But the special attractions were the new Upward's primary battery and an entirely new gas-burner, to be known as the "Welsbach." The Upward's battery, then lately brought out, is quite a new departure, completely dispensing with the use in the cells of acids and depolarising solution, chlorine gas being used as the electrolitic instead of a liquid. The elements used are the usual carbon and zinc. Each cell consists of an inner and an outer vessel, the outer one being of glazed earthenware with air-tight inlet and outlet pipes, and the inner an ordinary porous pot. The carbon is placed between the two vessels, and the top of the chamber sealed with cement. In the inner pot is a plate of

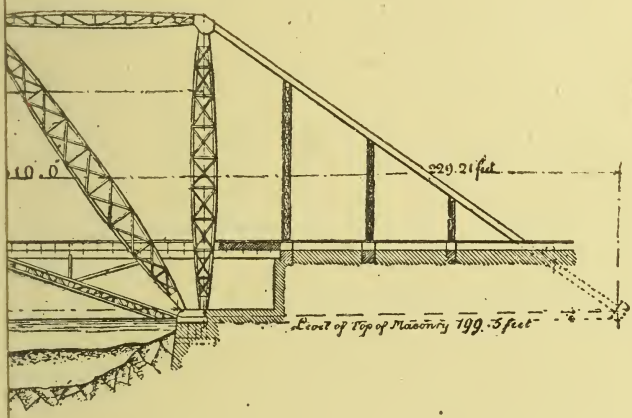
ordinary commercial zinc, the pot being then filled up with plain water. In working the battery, chlorine gas is admitted to the outer chamber and circulates from cell to cell. There is no escape of the gas, and the arrangement of the generator is exceedingly simple, so as to require no skilled attention, and to be perfectly free from smell. It can therefore be used in an ordinary house without difficulty. The principal applications are to electric lighting, driving electro-motors, as for fans or punkahs, supplying the current for electric bells, &c. It can also be used advantageously for electro-plating and other manufacturing purposes. One chief merit is self-adjustment, through the circulation of the chlorine gas, of the electrolytical action to the power being given out, so that not only is the zinc not being used up when no work is being done, but lights can be switched off or on without at all affecting the steadiness of the remainder. The visitors were much pleased with the fulness and steadiness of the light given by a number of glow lamps, arranged to exhibit the results of the battery.

The Welsbach burner was, if anything, still more interesting, both as a novelty not yet actually brought out, and for its intrinsic merits. It consists of a Bunsen burner over which is suspended a small bottle-shaped bag made of Brussels net, rendered non-inflammable by being dipped in a special solution. When the gas is lighted, this net bag becomes incandescent and gives out a steady and bright light. It is stated that the result is as much as 7-candle power for every cubic foot of gas, and that the bag lasts about 1000 hours, and then only requires changing because of becoming fouled, not because it is consumed. The details of the various operations and apparatus were explained by Mr. Allen, the manager of the works, and Mr. Upward.

The fourth and last of the Vacation Visits was made, on the 21st of September, to the London Yard Engineering Works of Messrs. Westwood, Baillie & Co., its object being to see at a late stage of construction the cantilever of the great Sukkur Bridge, designed by Mr. A. M. Rendel, of Westminster, which was being commenced at the time of the Society's visit the year before. Recapitulating the main dimensions and general description, as given in the 'Transactions' for 1885; the main span of this bridge is 790 feet in the clear between the abutments, and the vertical pillars are 820 feet apart, centre to centre. The space between these pillars is crossed by two cantilevers, of 310 feet each, connected by a girder bridge 200 feet in length. Each cantilever springs from a pair of pillars, framed and braced, 174 feet high, vertical on the side elevation, but inclining transversely towards each other. From

the same base-plate as these pillars there rise a pair of raking struts connected with them on top by horizontal ties, which carry on the tension from the inclined ties stretching to the end of the cantilever to those over the land which terminate in the anchorage. The girders carrying the roadway, which form the compressional members of the cantilever for more than half its length, are 40 feet above the level of the base-plate, down to which the compression is carried by inclined struts, leaving the horizontal at about 110 feet from the pillars. This general description will be made clear by reference to the accompanying elevation of the bridge, which also shows the minor members completing the structure.

At the time of the Society's visit, the great bedplates, the main pillars, and top horizontal ties were in place, and the main struts about half erected. The whole of the roadway girders, main and cross, and much of the flooring, were fixed, as also the inclined bottom booms which spring from the bed-plates. There was also an immense amount of work only waiting erection, including many parts of the second cantilever, which was being pushed forward simultaneously with the first, so that there should be no delay in erecting the second as soon as the first cantilever was taken down for shipment. Among other specialities of this bridge which were objects of particular interest were the corner plates of the main pillars and struts, bent cold to a quadrant of 6 inches radius transversely, and to the sweep of the pillar or strut longitudinally. As stated in last year's 'Transactions,' the material throughout is steel, the particulars of the specifications and tests of which were also then given. The first cantilever has since the visit been completed and shipped.



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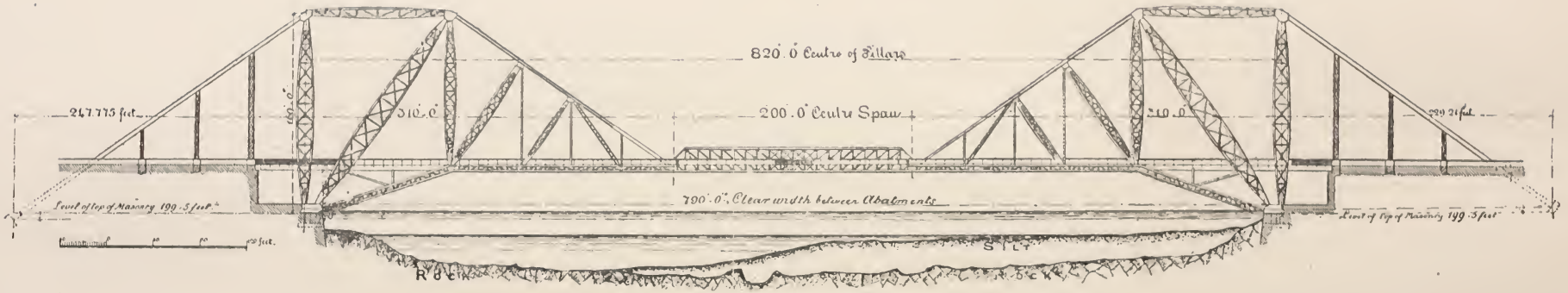
THE SUKKUR BRIDGE.



Designed by Mr. A. M. RENDEL, M. Inst. C.E.

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October 4th, 1886.

PERRY F. NURSEY, President, in the Chair.

THE LARTIGUE SINGLE-RAIL RAILWAY.

By F. B. Behr, A.I.C.E

In bringing the subject of the Lartigue single-rail railway before the Members of the Society of Engineers, the author would premise that a single-rail line is by no means a new idea. Since the commencement of the present century various engineers have endeavoured to carry out the principle of constructing a railway with a single rail. But although numerous patents have been taken out, and although various systems embodying the idea have been tried, not one has been carried out to a successful practical issue. Telford, in one of his works, suggested that this was the correct principle on which to construct cheap railways, and Palmer took out patents in 1823, and attempted to construct a single-rail line in London, which, however, was a failure. Other engineers in England, France, and America, have also taken out patents in this direction, but have always failed when they came to work out the details of their schemes.

It is only stating a truism to say that the material progress of a country will be in proportion to its railway accommodation, that is, of course, if the lines are judiciously laid down. But railways, although they are the greatest factors of advancement, involve serious expenses, in construction, in maintenance, and in working, which can only be borne by rich, populous, or productive districts. Engineers have long directed their attention to the discovery of the best means of supplying small places with some practical means of locomotion, and decided advances have already been made in the introduction of narrow gauge railways with steam or animal haulage. The cost of construction has been reduced in proportion to the smaller size of the line and the plant, and many disadvantages have been greatly reduced. The principle, however, remains the same, and the works to be carried out on these light railways still are very similar to those on ordinary railways.

Cable transport has been adopted, particularly for crossing

deep ravines, but it involves a heavy first cost for construction, and can never be used for a line of any length, as the motive power is stationary, and must be at least at one, and not unfrequently at both ends of the line. Cables of a diameter of $1\frac{3}{4}$ inches to 2 inches, and weighing about 20 to 30 lb. per yard, are frequently required over the whole length of a line on which the traffic does not exceed 100 to 150 tons per day of 12 hours. If to these expenses are added those of working and maintenance, the necessity of running for considerable distances in a straight line, and the consequent impossibility of following the inequalities of the ground, the outlay incurred on account of the necessary machinery which, being fixed at the ends of the line, is often of very large size, even where the work to be done is comparatively insignificant, some idea can be formed of the great cost of this means of transport. Nor can this system ever be used for the conveyance of passengers, for the cables, however near together the supports may be placed, are always extremely elastic, a serious disadvantage, and one which can only be overcome by increasing the loading of the cars. This increase in the load requires in its turn a stronger section of cable, and consequently an augmentation of the weight and of the cost. Moreover, the cable, however well stretched it may be, falls by its own weight into curves, producing a fall and a rise between the points of suspension, and entirely destroying the evenness of travelling.

It is claimed for the Lartigue system that the details have been brought to such perfection, that lines upon the single-rail principle can be made and worked under all possible circumstances, and can be utilised for every known purpose of transport. Elaborate and costly experiments extending over some years have had to be carried out before the present results were arrived at, and the system has been successfully applied in practice for lines worked by animal power, by electricity, by steam, and on a small scale by compressed air. The great feature of the system is that whilst always retaining the same principle and the same simple arrangement of the main parts, these can be so modified as to adapt themselves specially to every possible kind of work, the construction being thereby carried out with the greatest economy, and the cost of both working and maintenance being brought down to a minimum.

Before proceeding to describe the construction of the system, the author will briefly point out how M. Lartigue was led to invest it with its present form. The transport of esparto in Algeria from the extensive plains on which it grows, to the main lines of communication, is one of the most important

features of the trade, and M. Lartigue spent much time in endeavouring to effect this transport in the most economical manner. Whilst seeking the solution of this financial problem, M. Lartigue conceived the idea of his railway, and it was suggested to him in a very curious manner. The appearance of a caravan of camels on the horizon following one another in a long string and laden with thellis (a kind of wallet which hangs down on each side of them), gave him the first idea of an elevated rail. To his mental view the legs of the camels became tressels, their humps were transformed into wheels, and the thellis took shape as a car. The idea was rapidly matured, and the first line was made in Algeria. It was constructed of very primitive form, but improvements rapidly succeeded each other, and before long there were more than 60 miles of the Lartigue single-line railway transporting esparto in Algeria.

The example thus set there has been followed by the Anglo-French Company for working the esparto fields of Tunis, which is authorised to lay about 60 miles of Lartigue railway. Thirty miles of this railway have just been completed, and are in successful operation. They are worked by animal power—mules being the animals employed.

The Lartigue system has also been applied in an important manner in France at the mines of Ria, in the Pyrénées Orientales, where copper ore has to be carried a distance of about one mile from the first mine belonging to the Ria Company. The motive power here is electricity, which does not render any important modifications of the Lartigue system necessary. The line is working very satisfactorily, the management of the electric trains being entrusted to the ordinary workmen. The loaded trains go down inclines as steep as 1 in 12, and only empty trains are taken up hill. This peculiarity has been utilised in a very novel manner. The electricity is entirely generated by the friction of the loaded trains going down hill, and is sufficient to take the empty train up, so that the electricity is produced without any expense, and it is believed that this is the only case where an arrangement of this kind has been carried out.

In 1884 the Lartigue system was exhibited at the General Agricultural Exhibition in Paris, and at the Rouen Exhibition, where a short line was laid down and worked in the Exhibition grounds. A considerable length of this line was also laid down and worked at the Exposition du Travail in Paris last year. In all these arrangements the motive power consisted of a small Siemens dynamo placed on one side of the leading car, and balanced by the driver who sat on the opposite side. In

Russia also some very satisfactory trials have been made by a committee of engineers and engineer staff officers specially appointed for the purpose by the Russian Government to test the system, not only for general purposes, but more especially with regard to its applicability for military purposes. The results were most satisfactory, and the report of the Commission was in every way favourable to the adoption of this system in Russia for military purposes as well as for railways traversing enormous distances through the steppes of Central Asia.

The following is a short extract from the report of the Commission:—

"To give an idea of the important results to be obtained when it is possible to use gradients of 1 in 17; in a length of 660 feet a difference of level of 40 feet was obtained, and in a distance of 3300 feet a difference of level of 200 feet. It is easy to understand the advantage that is to be gained by the use of this system in our Asiatic possessions and on the steppes, where it is at present necessary to use large numbers of beasts of burden for military transport. In that particular case the Lartigue railway presents advantages which can only be described as incalculable."

Although both animal power and electricity have proved successful in working the single-rail system, it was felt that neither of these methods was suitable for lines of great length where speed is required, nor for the carrying of a mixed traffic of passengers and goods of a certain importance. It was therefore resolved to design a line to be worked by steam, and in April last the details of such a line were settled by the author in conjunction with M. Lartigue and Mr. A. Mallet the well-known French locomotive engineer. It was determined to construct a full-sized working line in London, and for this purpose a plot of ground on the site of Tothill Fields Prison at Westminster was secured, and the construction at once commenced. The author would here point out that this is the first line constructed on a full working scale for passengers and goods traffic worked by locomotives on the Lartigue system, and that the conception, designing and construction of it were carried out in barely two months. Improvements in the details of construction have therefore suggested, and will doubtless further suggest, themselves by practical experience, though the line at Westminster has fully proved the ease and economy with which lines on this system can be constructed, and the safety and economy with which they can be worked.

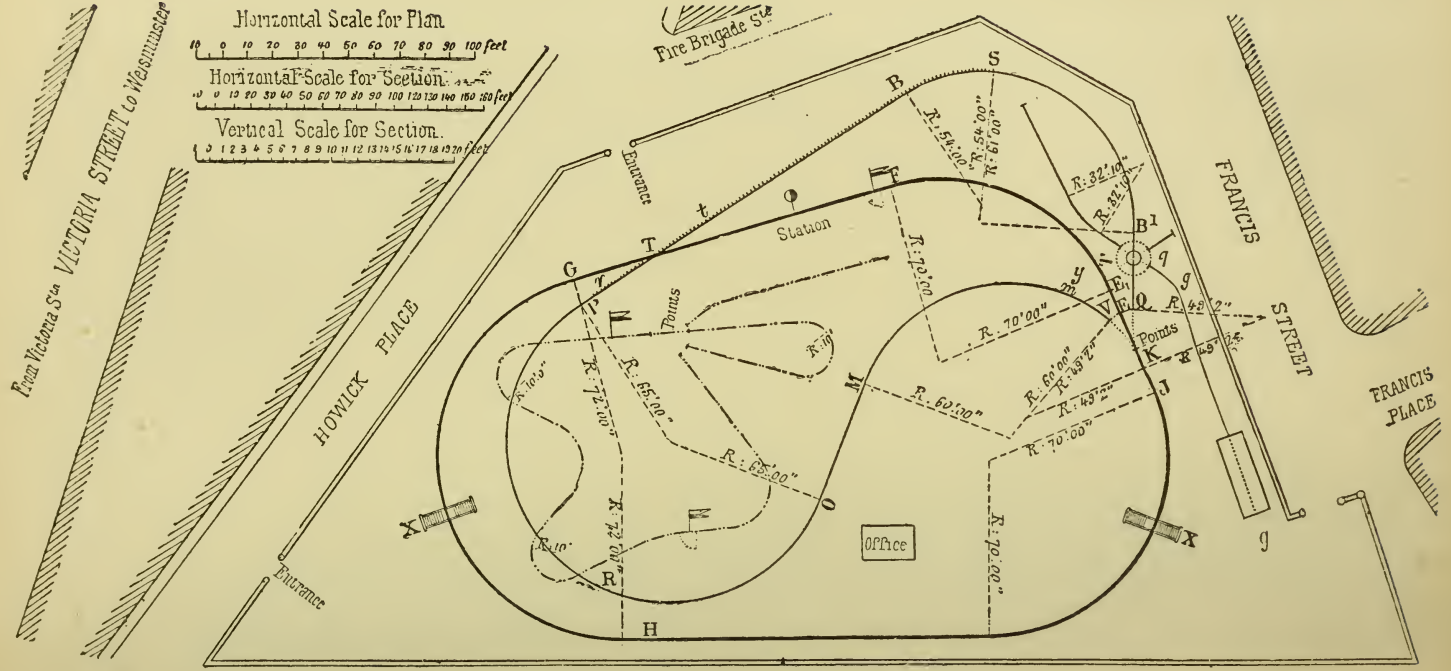
The Lartigue system is principally intended to give railway accommodation to poor and sparsely populated districts, as well as to afford facilities for military, agricultural, mining, and

industrial transport generally. The line, which is made portable, when applied to military, agricultural, or manufacturing purposes, is, when reduced to its simplest form, a line composed of one rail supported above the ground by A-shaped tressels or frames, made of angle iron. The upper part of the tressel is bolted to the rail, and the lower extremity rests on the ground, being supported by an iron sleeper, to which the frame is secured. The sleepers are kept in their places, where necessary, by pegs driven into the ground through holes drilled near the extremity of the sleepers. The tressels are strengthened by struts supporting them in the plane of the railway. The rails are connected by fish-plates, one half of each pair of fish-plates being secured to one end of each rail by two bolts or rivets. Two more bolts or rivets connect the other extremity of the pair of fish-plates, passing through holes made for that purpose. The end of the next rail is provided with two slots on the under side, which fit on to the open end of the pair of fish-plates. In order to make the joint, the end of the rail is slightly raised so that the two slots come over the two bolts between the fish-plates, and after letting it drop into its place, the bolts are tightened, and the joint is secure. Two of the holes in the fish-plates are oval, to allow for expansion. As the rail is flat and laterally flexible, curves are made by hand, in the following way. The line is slightly raised, one man keeping it steady at the place where the curve is to begin, whilst another shifts the free end of the rail in the required direction; the line is then again dropped on to the ground and the curve is made. The direction of the line can be changed, if required, in the same way and with equal facility. Shunting is carried out without any special apparatus, as the line thus constructed contains in itself all that is required for the operation. A free joint at the point of junction allows of the slotted end of the rail being raised from between the fish-plates of the one line and dropped in between those of the other, exactly as if the line was in the course of construction, and the shunting is completed.

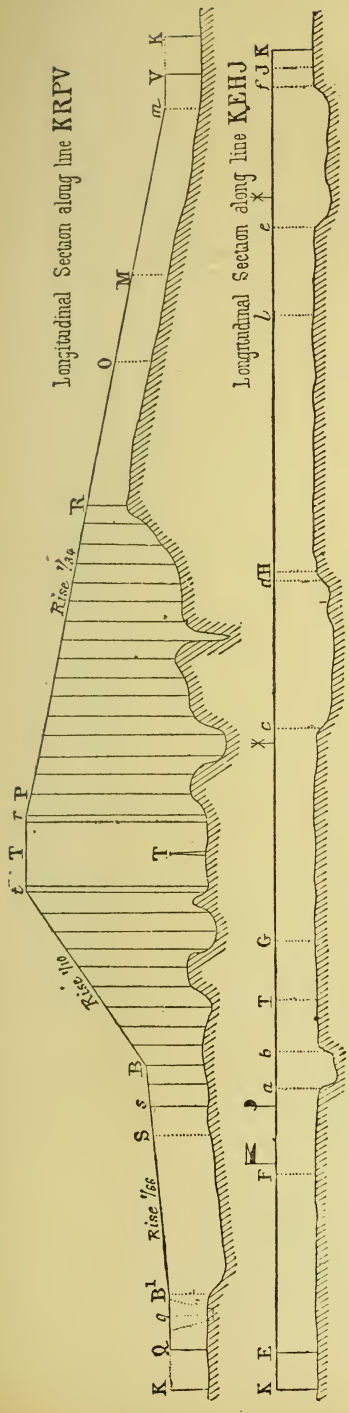
If a river has to be crossed, some light piers can be made, if the amount of traffic warrants it, or two wire cables can be stretched across to receive the tressels of the line, or it may even be sufficient to construct the tressels of different heights so as to carry the line over, the base of the tressels resting on the bed of the river. If, on the other hand, a ravine has to be crossed, the line can either be carried over it as it would be over a river; or it can be taken down the side of the gorge by means of zigzag lengths, which can be connected by curves of as small a radius as ten feet.

The above description gives a general idea of the Lartigue

FIG. 1.


Horizontal Scale for Plan
10 0 10 20 30 40 50 60 70 80 90 100 feet
Horizontal Scale for Section.
0 10 20 30 40 50 60 70 80 90 100 120 130 140 150 160 feet
Vertical Scale for Section.
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 feet
From Victoria Sta. VICTORIA STREET to Westminster
HOWICK PLACE
Fire Brigade Sta.
Entrance
Station
Points
Office
FRANCIS STREET
FRANCIS PLACE
R: 54'00"
R: 61'00"
R: 32'10"
R: 70'00"
R: 65'00"
R: 72'00"
R: 60'00"
R: 43'2"
R: 49'2"
R: 10'
R: 24'00"

PLAN OF RAILWAYS BUILT AT TOTHILL FIELDS, WESTMINSTER.



SECTIONS OF RAILWAYS BUILT AT TOTHILL FIELDS, WESTMINSTER.

system when applied to portable lines, and even into this simple construction improvements have been introduced since the line was laid down at Westminster; for instance, most of the bolts have been replaced by keys which are driven in by a hammer, an improvement which may be of importance when the lines are sent to distant countries or the colonies, where the ordinary native labour might have to be employed. In permanent lines, however, particularly in those on which it is intended to convey passengers, the arrangements are somewhat less rudimentary, as will presently be shown. Guides to prevent the swinging of the cars, and points, sidings and signals, &c., have been introduced, but everything has been done with the same regard to simplicity.

The general arrangement of the single-rail lines constructed at Westminster will be seen in plan and sections on Fig. 1. They illustrate the two types already referred to, namely, the permanent passenger line and the portable line for military, agricultural, or industrial purposes. The first line K E H J is practically on the level, and is employed in speed trials and for determining the consumption of water and fuel. The second line K B M V is irregular, with curves of as small a radius as 49 feet,

and with gradients as steep as 1 in 10. There is a level piece from Q to B′, on which a turntable is fixed. From B′ to B the line rises on a gradient of 1 in 66, on which there is (between S and B) a curve of 54 feet radius. The manner in which the construction of embankments may be dispensed with, is illustrated by increasing the height of the tressels in some parts of this line from 3 feet 3 inches to 5 feet. A rack rail, which will only be used to ascend steep inclines, has, in the present instance, been laid from the point S, so as to have a certain length on the ordinary line, for the purpose of showing how the junction between the two is carried out, and how the former is treated when it has to be laid on curves. A wooden viaduct, about 340 feet long, commences at S and ends at R. From B to *t* the line is straight with a gradient of 1 in 10. At *t r* one line crosses over the other on the skew, and in consequence of this obliqueness there is a bridge of 33 feet span. From *r* to *m* the line descends with a uniform inclination of 1 in 34. From *r* to R the viaduct continues on a curve of 65 feet radius, and from R the ordinary line goes on with its tressels 3 feet 3 inches high. From *m* to V the line is horizontal, and a junction is effected with the line K E H J by means of the three-throw switch V K Starting from E on this line, there is a level crossing at F near the station, at T the crossing of the two lines, at X overhead crossings for foot passengers over the line, and then the line returns to the switch. These two lines, which are of about equal length, are in all 1540 feet long.

It will be seen that every possible feature has been introduced into this experimental line, in which difficulties have been purposely exaggerated. Inclines upwards and downwards, sharp curves, junctions, switches, level crossings, everything in fact is to be found, and the means of overcoming any obstacle demonstrated. The viaduct takes the place of a heavy piece of work which frequently cannot be avoided in laying out a railway, but which on a single-rail line is reduced to what may be called its simplest form. Here the uprights of the viaduct itself serve also as tressels, and the iron supports are dispensed with. The introduction of tressels of different heights suggests a means of avoiding embankments, and of crossing any ordinary inequalities in the ground, such as ditches, ravines, and brooks.

Shunting is effected by the lateral flexibility of the rail. The side guides are jointed, and there is a certain amount of play between them, so that they alter their relative positions when, by the working of the switch, the curve on one side is changed to the curve on the other side. The switch is formed

by a piece of line 23 feet long with tressels of the usual height. The sleepers of each tressel are fitted with two runners, which move on a metal track placed on a wooden framework. A bolt, fitted to two of the sleepers, drops automatically into a slot prepared to receive it, at the spot corresponding to the position of the switch when it forms a straight line, and locks it during the time the train is passing. These bolts are connected together so that they can be wedged in their place when they are down, and raised by a lever placed at the end of the switch lever.

When the switch is shifted so as to form a curve of 49 feet radius to connect with the line either on the right or the left of the straight line, the switch is held in its proper position simply by a bolt at the end of the rail and by the pressure exercised upon it by the train passing over it. This switch has been considerably modified since it has been placed in position on the ground and now works most admirably. The free end of the movable rail is fitted with locking fish-plates, whilst the extremities of the rails of the fixed lines have a tongue which fits between the fish-plates. By this arrangement the switch is perfectly secured, and the train passes over the points with as much safety as over any other part of the line. The switch is worked from the straight to the curved lines by pulling or pushing the lever after having, by means of the handle, withdrawn the bolts from the slots, the lever for the points being connected with the signals beyond the points, which show which of the three lines is open.

The opening of the level crossing is formed simply of one length of the line of the same dimension as the space required, which turns on a hinge fitted at one side of the road. The other extremity is fitted with runners which work on a metal track laid on a wooden framework, and the movable rail has at its extremity the same fish-plate catch as the points. It is only the last tressel which is fitted with runners, the sleepers of the others being shod with wood, which slide on the framework already mentioned. When the movable rail is open it rests only on the runners of the extreme tressel, but when it is closed these drop into hollows arranged to receive them, and the rail takes its bearing on all the tressels. The opening and shutting of this gate sets in movement a signal on either side of the level crossing, which shows by its position if the line is open or closed. The turntable is composed of a length of 13 feet of line fitted on two longitudinal beams, which are in their turn carried by an ordinary turntable of 6 feet diameter. The ends of this length of line are fitted with the fish-plate catch in the same manner as the points and the level crossing.

The traction on the Westminster line is effected by a special locomotive invented by Mr. Mallet, and shown in side elevation at Fig. 2. This engine is composed of two vertical tubular boilers A A, placed one on each side of the line and connected

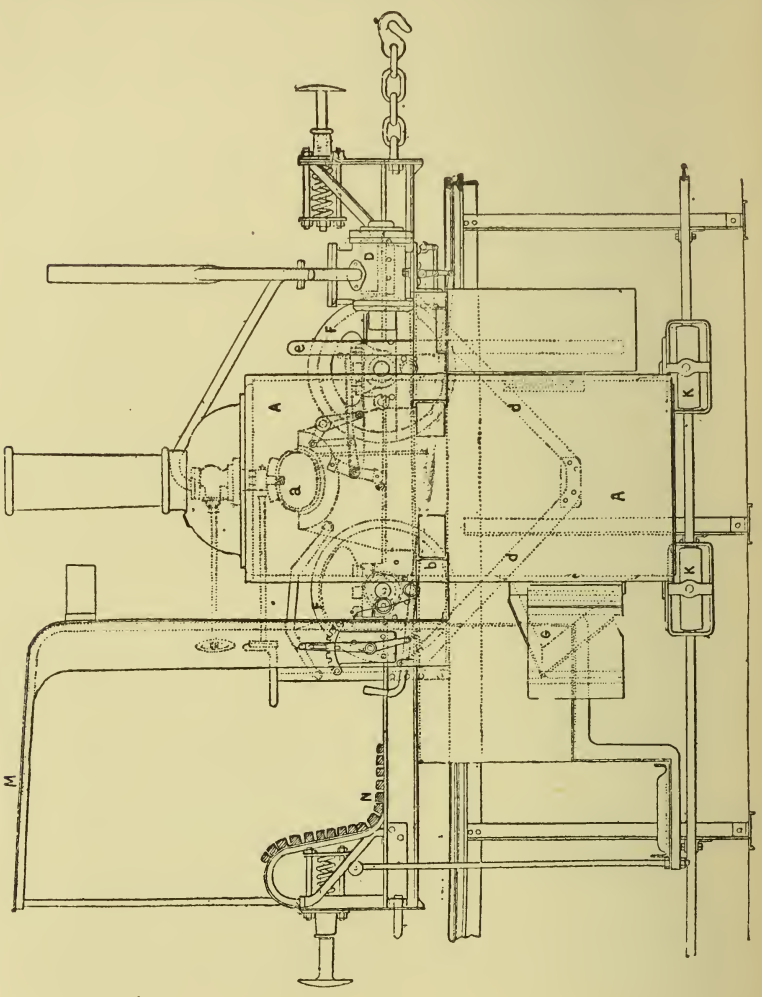


Fig. 2.

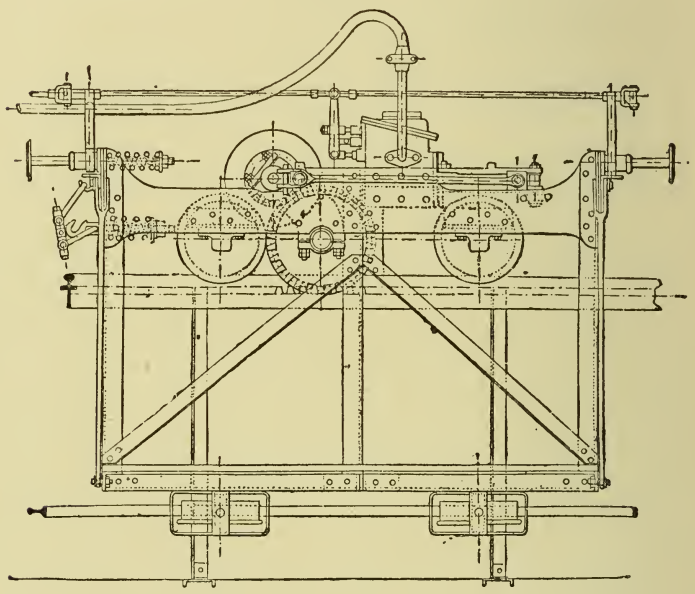
at one end by a large pipe *a*, which both acts as a steam dome and carries the two boilers on the framework of the machine; and at the other by a pipe of small diameter, which allows the water to pass from one boiler to the other. This latter pipe is

placed at a height at which there is always water, so that it is never empty. By this arrangement it is sufficient to feed only one boiler, which supplies the other, the pipe at the same time preventing the water in the inner boiler rushing into the outer one from centrifugal force, when the engine passes rapidly round a curve. The safety valves, whistles, &c., are fitted on the large pipe *a*. The engine is carried by two grooved wheels F F, on a framework to which the boilers are attached by the bars *b*, and the diagonal stays *d*, as well as by the steam pipe *a*, as already stated. The horizontal cylinders D D, which occupy a position at the front of the engine, drive the cranked axles of the wheels, which are coupled, in the usual manner. The firing is done by the engine driver, by means of hoppers fixed at the rear of the boilers. The driver, sitting astride on his seat N, and protected by the roof M, can easily throw into the hopper the coke, which he takes out of the bucket fixed at the side of the engine. The feed water is contained in tanks placed in front of the boiler and connected by a syphon which reaches to the bottom of each boiler and prevents any difference of level in the water in either tank. Although the engine is balanced by its construction, since it is symmetrically even with the plane of the line, in order to avoid its being thrown out of equilibrium by any accident or by centrifugal force when going round curves, horizontal wheels, K K, running on longitudinal guides attached to the tressels which carry the line, hold the engine in a vertical position and prevent it from canting over to either side. In working order this engine weighs about $2\frac{1}{2}$ tons; that is $1\frac{1}{4}$ ton on each axle. The wheels are 15 inches in diameter, and the cylinders $4\frac{1}{2}$ inches, with a 7-inch stroke. The boilers have together a heating surface of about 70 superficial feet. With a steam pressure of 100 lb. to the square inch the engine will haul about 70 tons on the level, 18 tons on an incline of 1 in 100, 9 tons on an incline of 1 in 50, and 6 tons on an incline of 1 in 33, at a speed of 5 or 6 miles an hour, which, with a smaller load, can be increased to 10 or 15 miles an hour. This engine can easily go round curves of 30 feet radius.

The locomotive is fitted with a powerful hand-brake, which is sufficient to stop the train whilst the carriages are attached to the locomotive; but, to prevent accident in case of a breakage of the connections between any of the carriages, the Westinghouse Brake Company have fitted the train with their continuous automatic brake. The apparatus used on the engine is only temporary and much too large. The Westinghouse Company, however, propose to design special brakes of the same type as those they supply to ordinary railways, but reduced to the

proper dimensions for the class of engines used in the Lartigue system. The carriages have already been fitted by the Westinghouse Company with a very ingenious set of apparatus, especially modelled so as to fit the size of the rolling stock adopted on this line. In the present instance the Westinghouse air-pump and reservoir are placed on an auxiliary machine in the rear of the locomotive to give it weight and steadiness. This machine (Fig. 3) consists of a steam cylinder driving a spur wheel which gears into the rack rail and assists the ascent of steep inclines. The whole is mounted on a pair

FIG. 3.



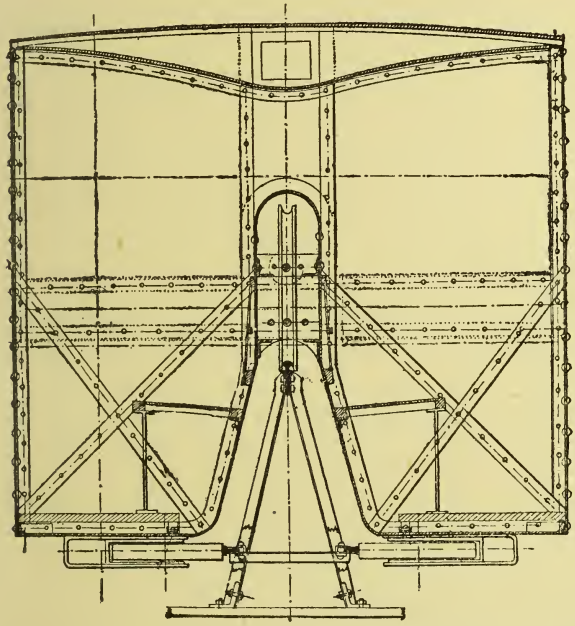
of grooved wheels, which run on the single rail. Like some other details of the system, however, this apparatus is more or less experimental, and the author need not further refer to it. These then are the principal features of the steam-traction apparatus at Westminster, which is of small type and is intended only for very light work.

The carriages and waggons are of an entirely new design, and are suited to work a long line. All the necessary features of strength and steadiness which should be present in rolling stock on a working railway with a developed traffic have been introduced, with one omission the reason for which will be hereafter

explained, namely, that neither the engine nor any of the carriages or waggons have been fitted with springs.

Figure 4 represents a transverse section of the iron framework of one of the carriages. It also shows the arrangement of the tressels carrying the rail. The framing is composed at each end of a strong wrought-iron bar, arched above the rail, which, as it is prolonged downwards on each side, runs parallel with the tressels of the line until it reaches within 1 foot 3 inches of the ground, when it turns outward and becomes horizontal. This last portion carries the flooring of the carriage; the part

FIG. 4.



which is parallel with the tressels forms the back of the seats and the arched upper part above the rails leaves the necessary space for the wheels and brakes, and admits of their examination. To this part is attached a horizontal wrought-iron bar, which stretches out from the line on each side as far as the outside of the flooring of the carriage. By uniting this flooring with the horizontal bar, by means of iron diagonal stays, a stiff body of uniform shape and quite symmetrical as regards the line is obtained. This forms an excellent framework, to which panels either of ornamental wood or painted and varnished sheet iron can be fitted according to requirement. The

carriages are fitted with horizontal wheels placed underneath on each side, and which run on the same guide rails as the horizontal wheels of the locomotive, giving a steadiness to the carriage which no accidental occurrence can disturb. The carriages are attached to each other by the usual coupling hooks, and are fitted with a special central buffer. On the Westminster line is a very useful form of carriage, which, arranged in the first instance to carry goods, can, when required, be altered to carry passengers. It is fitted with a shifting seat, which is fastened up when goods are carried, and let down if the carriage is required for passenger service. By this arrangement any special pressure on either department of the rolling stock can be met without increasing the number of carriages. This carriage is protected by a movable awning.

The ironwork for the construction of the railway, such as the rails, tressels, level crossings, turntables, and switches, was manufactured at the works of M. Achille Legrand at Mons. The rails were specially rolled for this line. The locomotive and the small auxiliary machine to work the rack were constructed at the works of La Métallurgique, at Tubize in Belgium, and the carriages by the Société des Forges et Ateliers at Saint Denis, near Paris.

The engine and rolling stock were constructed without springs, and the road laid down very roughly at the joints of the rails, in order to put the railway to the most severe test. The trains have been run every day from 9.30 A.M. to 5.30 P.M., and the line has admirably withstood the severe and continuous strains and shocks inflicted upon it by the absence of all springs. This has fully demonstrated that the construction of this railway is such as to need very little repairs for a considerable period when worked in the usual manner, as the shocks inflicted upon it during the three months were much greater than those it would be subjected to during several years when worked with a properly constructed rolling stock.

At Fig. 5 is shown a special form of tipping waggon for use on Lartigue mineral lines. It can also be used for removing soil on earthworks, where it will be found very useful owing to the rapidity with which its contents can be discharged. But above all would this waggon be useful for automatically tipping from the Lartigue line into the trucks of main lines, to which these light lines would act as feeders. An arrangement has been designed by which the Lartigue line could be carried over the main lines, in such a way as to tip automatically into trucks on the main line. These waggons are so constructed as to be easily handled and shifted over any track of the main railway, to facilitate the loading of the ordinary trucks. The same

arrangement is applicable to loading ships. Another light line has been designed for harvesting grapes, a process which is at present very costly, as the grapes have to be carried by hand to the carts which remain outside the vineyards. The arrangement is such that, as the hands proceed harvesting, they drop the

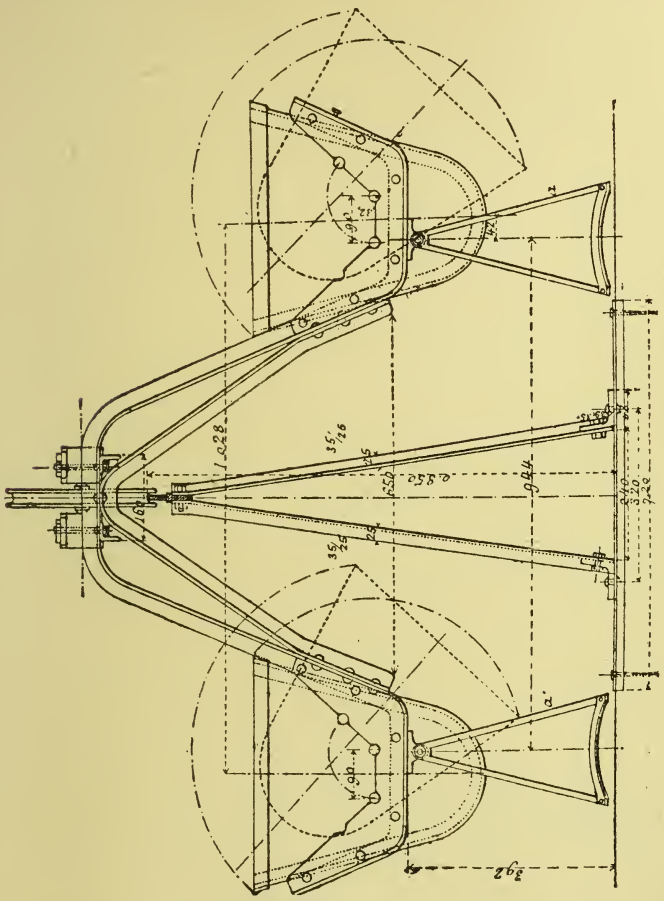


FIG. 5.

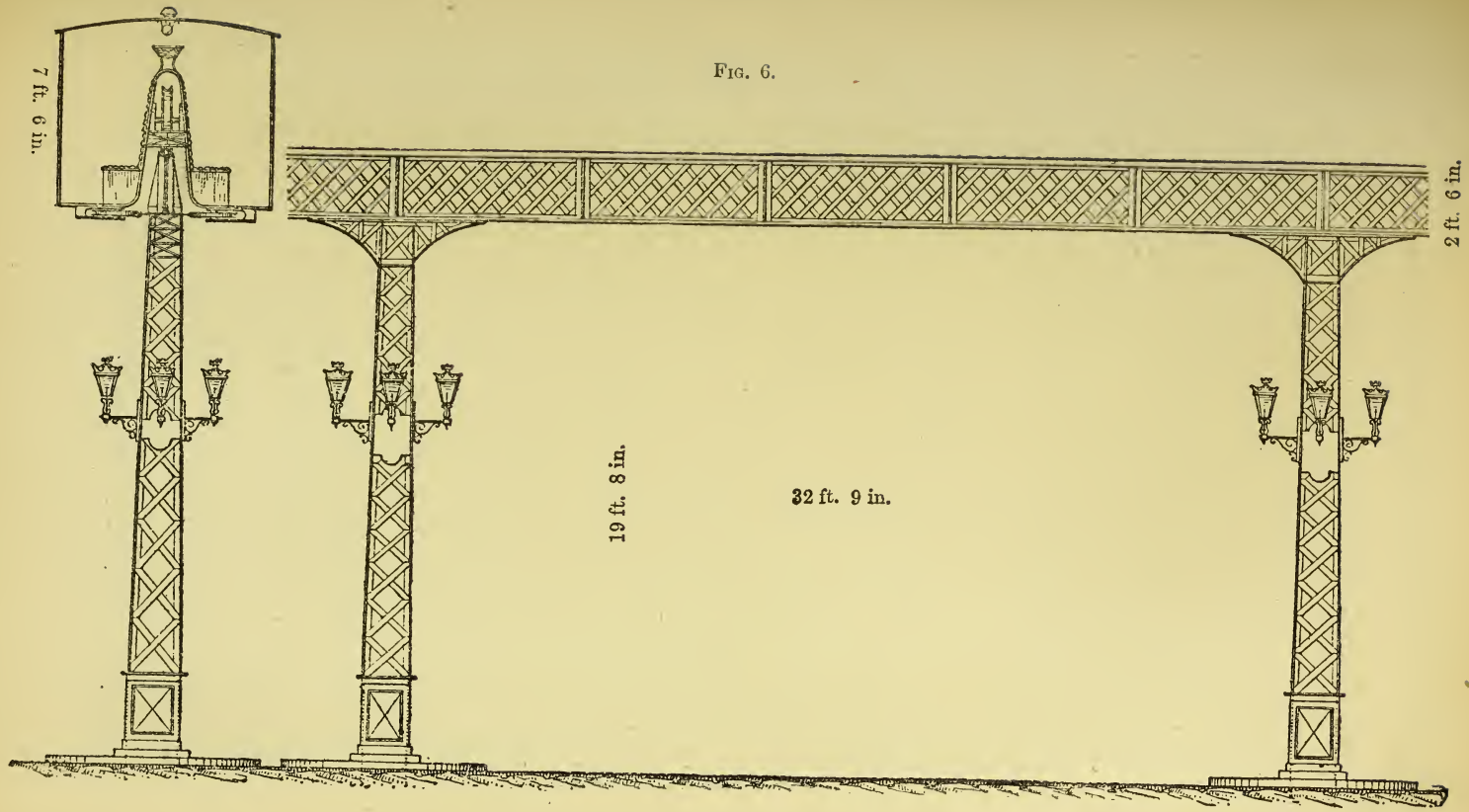
grapes into trucks, which follow them on through the operation and are then pulled back by a rope along the rail out of the vineyard. A similar arrangement has been designed for the harvesting of sugar-cane on the plantations.

Such is the Lartigue railway system with its accessories, and in considering its advantages the author submits it is eminently

calculated for use in the directions he indicated in his opening remarks. It affords a means of connecting important manufactories with the main lines of railway, which connection is at present impracticable by reason of the heavy expense attending the construction of the lightest ordinary line. Agriculture of all industries would certainly be most largely benefited by the Lartigue system, for agriculture often requires long stretches of line, which have to be frequently shifted with few and unskilled hands, and with the most primitive plant. For military purposes the line would be invaluable, and ought to form a part of the permanent equipment of every army, as it can be packed in a very small space, and divided into light and easily handled packages. The line could be erected in an emergency in a very short time by unskilled workmen without any previous teaching, and would facilitate the carrying of provisions and baggage, as well as the transport of troops and the removal of the wounded from the field.

A still more important question perhaps is its application in the construction of overhead lines, for replacing tramways in the suburbs of large towns, and in some cases in the towns themselves. Fig. 6 represents such a line in cross section and in elevation, one bay being shown. This in fact is the design proposed for a metropolitan line in Paris, and also for carrying visitors to and from the different parts of the Great International Exhibition to be held in Paris in 1889. It simply consists of light columns placed at frequent intervals and connected by a girder which forms the line. Such a line can be carried through any thoroughfare of moderate width, by constructing it along the centre of the roadway in a single line. The space really occupied would only be that of a large lamp-post placed every 30 or 40 feet. These columns will only have a width at the base of 2 feet 6 inches, and at the top of about 1 foot 4 inches. It is proposed to have a clear height above the roadway of 20 feet, and on the tops of the columns will be carried a lattice girder of a depth of 2 feet 6 inches, which supports the rail. An elegant appearance can be given to the structure and the columns will serve as lamp-posts, having four lamps each, thus lighting the roadway better than by any present arrangement. The space round the bases of the columns would at the same time serve as refuges. There is hardly any important street in any town which is not sufficiently wide to spare at every 40 feet in the centre sufficient room for a lamp-post 2 feet 6 inches square. The system would be laid down in circles so as to enable a continuous service of trains to be run, thus dispensing with passing places and with double lines. The trains would be sufficiently far removed from the houses not

FIG. 6.



to interfere with them in any way. This system would relieve the very congested state of the traffic on the surface of the streets in many localities without entailing the enormous expense of underground lines. This is a problem which has often been discussed, but has not been solved with such simplicity and at such little cost by any other system of railway. The cost of maintenance in the Lartigue system is also reduced to a minimum, and the heavy expense of repaving the streets in which tramways are laid is dispensed with, thereby yielding an increased return on the capital invested.

A line on this system could be built in a city like London at a cost of about 5000*l.* per mile, with supports at a distance of 35 feet apart, and with the line at a height of 20 feet above the roadway. It could be worked either by electricity or by steam; in fact, the line already referred to as proposed to be built on this principle in Paris, as well as for the Exhibition of 1889, is to be worked by electricity, and a company has undertaken to work it for the Government at a fixed rate per mile and per ton, under a very considerable penalty. The line for the Paris Exhibition is designed in four circles—two of them having a common point of contact where a station is to be established. It is calculated that this line will be able to carry 150,000 passengers per day.

The author would again observe that, since the construction of the line in Westminster, great improvements have been made in the construction, not only of the permanent way, but also of the engines and carriages. These improvements have been carefully tested, and the author hopes to be able to make other improvements to still further simplify and cheapen the construction. For the present it is sufficient to say that the weight of the line has been reduced from 75 tons to about 50 tons per mile, and this in such a manner that instead of the line being weakened, it will, though so much lighter, carry weights about one-third greater; that is to say, if the line as at present constructed will safely carry, say, 6 tons on a carriage with four wheels, it will carry 8 tons in the same carriage on the improved line with the weight of structure reduced to about 50 tons per mile. Corresponding improvements have also been introduced in the carriages, which will render them more comfortable to ride in and cheaper to construct.

In conclusion, the author would observe that practical steps have been taken to introduce the Lartigue system into this country. Application was made to Parliament during the present session for an Act to authorise the construction of a railway for carrying passengers and goods from Listowell to

Ballybunion, in Ireland, about 10 miles in length, to be built on the Lartigue system and to be worked by locomotives. This Act received the Royal assent on April 16, 1886. Since then another Bill authorising the construction of a railway from a junction with the Great Western Railway to Lynton, in Devonshire, for the carriage of passengers and goods, to be worked by locomotives on the Lartigue system, received the Royal assent on the 25th of last month. This railway is of a length of about 30 miles. The author therefore hopes that at no distant period two lines will be constructed and at work in Great Britain, which will further demonstrate the value of the Lartigue single-rail system with the application of all its latest improvements.

DISCUSSION.

Mr. Hamilton Pendred thought that all who had listened to the paper must have been much interested. At present, there were two great systems—ordinary roads and great trunk railways—and for many years past the great want had been some system which would bridge over the chasm between the two. He had had some experience in attempts at wire-rope carriage of various kinds. Under special circumstances they did tolerably well, but there was one weak point about them, the wear and tear of the rope. They were cheap to make, but soon perished, and even the best steel ropes could not be worked economically. The Lartigue system seemed to be well worked out so far. No doubt, as had been suggested, little improvements would come in, but it seemed to him to be wonderfully perfect, considering the short time it had been in existence. He was rather surprised that Mr. Behr had failed to notice three branches of industry to which it would be eminently applicable: the carriage of coffee in Ceylon, the carriage of peat, and the carriage of beetroot from the fields to the sugar factories. There were many purposes for which peat could be used, if only it could be easily procured; the greatest obstacle arising from the nature of the ground, which slipped very rapidly and made it difficult for the means of transmission to follow it up. In his opinion Mr. Behr's railway came nearer to meeting this difficulty than any project he had yet known. For this purpose it could be made very available, and when a particular district was cleared out of peat, the railway could be taken away and used elsewhere, which could be done with no other system. In France, wire-rope railways were used for carrying beet: he was inclined to think the Lartigue system

would be more advantageous and likely to be cheaper. As to Ceylon, he knew that some years ago there were many inquiries as to some method for bringing the coffee from the plantations to the houses where it has to be prepared.

Professor ROBINSON thought the Society was to be congratulated on having so interesting a paper. The openings which would be afforded by this railway would be evident at once. Within the last few years he had been connected with two cases of proposed railways, in which the expense of constructing even a narrow-gauge line had been fatal to the execution of the work. Acts of Parliament were obtained in both cases, but after every possible effort on the part of the landowners and others interested they had failed in getting what they wanted. He was quite sure that if a line like this had been shown to be practicable and he had seen it at work, the schemes to which he alluded and which had been arrested would have had a chance of being carried out. The point which had occurred to him on seeing the line at work, with reference to passenger traffic, was that the overbalancing of the carriage on either side of the single rail would involve the guide wheels occasionally becoming a source of loss of power by friction. He saw the attendant that day taking great care to balance the passengers, the fat and the lean ones, and this had obviously to be kept in view. If half-a-dozen fat men got in on one side without being balanced on the other, the guide pulley would act with a great amount of friction on the guide rail, and this had to be kept in mind in calculating the power of the engine. In a discussion of this kind he thought that critical observations should be brought forward as well as complimentary ones. If this point had been provided for, he hoped the author would mention it.

With reference to the proposal to carry passenger traffic upon a line constructed upon columns, as shown on the diagram, with a base of only 2 feet 6 inches, he thought that a strain would be thrown upon the columns, which would be more than such a base would meet. With a skilful engineer like Mr. Behr, he had no doubt that this point was provided for—otherwise the structure would be subject to great strain and possibly to accident. Subject to these observations, he thanked the author for bringing forward so interesting a communication. He thoroughly approved the principle advocated, and hoped it would provide a means for developing the resources of poor districts, of which there were a great many in this country, which could not afford the cost of even a cheap tramway or narrow-gauge line, and that a large field would be opened to

engineering enterprise where the old systems were too expensive to be used. With reference to the cost, that must of course be subject to wide fluctuations. At the present price of iron it would be very much less than it would have been ten years ago, or might be in the future.

Mr. CHARLES GANDON agreed with Professor Robinson upon one point raised, which however had not previously occurred to him, namely, as to the friction upon the horizontal wheels at the bottom. Another point was as to whether in going round sharp curves the centrifugal force would not cause a great amount of friction upon one or other of the wheels, and he would like to ask whether any experiments had been made to ascertain if that was the case. The subject recommended itself: the desirability of a simple and cheap means of transport in sparsely populated districts was acknowledged, and this system was decidedly a step in the right direction. In its working, however, possible difficulties might be encountered which would be more apparent in a line upon a larger scale than that now being exhibited at Westminster.

Mr. CHRIS. ANDERSON said that no comparisons had been given as to the relative cost of this system as against the new light narrow-gauge lines made by agricultural implement makers, such as Fowler of Leeds, for sugar plantations and other places where skilled labour was impossible to be had. They were, however, dealing with a new departure; and he hoped the author would persevere, and that any criticisms would rather stimulate than discourage. With this object he would point out two or three things which had seemed to him that day to be serious objections. The first, which had been already pointed out, was the difficulty in balancing the load in the carriages. From the very construction of the carriage it was impossible to have a passage from one side to the other, and all that could be done would be to have a man there to arrange the passengers at starting, to put them in in equal numbers on each side and take care there was a proper balance. Even if this were done, the passengers at each successive stop or station would again require to be similarly regulated. There was no guarantee that the train would run on in anything like a balanced condition. Then came a question. Taking the view of the carriage as shown upon the diagram, and as they had seen it in working, and supposing it to be well packed on one side only, when it came to a curve at a moderate speed, if the passengers were on the outside of the curve, the centrifugal force would have a tendency to throw them still further out. To his mind, in the carriage

as constructed, from the centre wheel to the bottom there was nothing to keep the carriage from flying right out except the rigidity of the light framework of which it was constructed; and with the weight of the five people all on one side probably it would give way. At any rate it would put a very great strain on the side pulleys. Again, it seemed from the diagram that the bulk of the weight of the two boilers was borne by the steam pipe fastened to the frame of the carriage. Now in all work of this kind, particularly in agricultural work, it had been the aim to avoid fixing pedestals on the boiler shell. It was found utterly impossible to keep such work tight for any length of time. But it appeared to him that, unless the boilers were steadied by framework of a much more permanent character, there would be very frequent trouble with the joints. He did not agree with the author that it would not be much obstruction to have posts 35 or 40 feet apart in a main thoroughfare. Apart from the general objection to having in a street any overhead railway with traffic constantly passing overhead, the columns must be an obstruction, and he was afraid that local boards and town councils would strongly object for many years to come to anything of the kind being put up. At present if a railway company wanted to put a bridge across any main street, it was with the utmost difficulty they could obtain permission. With regard to the statement that pillars 35 or 40 feet apart would not be much obstruction, it was not the area they occupied that caused the obstruction, it was the simple fact that they were there, which would give rise to the fear in the mind of the driver that he might not get past in time to avoid fouling another vehicle. So that practically the effect of the columns would be to divide a street of average useful width into two single vehicle gangways, and thus considerably impede the traffic. He hoped the author would believe that there was no feeling in the minds of engineers against him. Nothing was more wanted by them than a simple, cheap and expeditious railway. He would like to have heard more about the cost as compared with those already in existence, which could be put down without skilled labour, and at even less than the cost of tressel work for which skilled labour was required.

Mr. H. Conradi said that one very advantageous point about the Lartigue system was that the whole machinery worked on a clean permanent way, which being above the level of the ground prevented the engine from getting muddy as on ordinary tramways, thereby effecting a great saving of wear in the mechanism. With regard to the balancing of the carriages he

had in his mind an arrangement by which the cars and engine could be kept in stability even if the cars were loaded only on one side. He did not wish to sketch out the idea he had. as it had only occurred to him during the reading of the paper, but he was pretty sure this difficulty could be overcome in a practical and simple way. With reference to the application of the system there was of course a distinction to be drawn between its application to tramways in towns, especially for passengers, and tramways in the suburbs and the country for passenger and goods traffic. He did not quite agree with the gentleman who said that columns 35 or 40 feet apart would be an obstruction. They would be so in streets like those of the City, but not in wide roads like the Commercial Road in the East End of London, where a cheap means of communication was most essential for and a great boon to the working population. He quite agreed that pillars with a base of 2 feet 6 inches were too small, but even with a base of 4 feet, or 4 feet 6 inches, he did not think the streets of many of our provincial towns or the suburbs, like the above-named and those going to Greenwich, were too narrow to allow such a tramway either on each side or in the middle of the road. It would at any rate be less an obstruction than the laying down of tram lines in the street itself. Tramways in themselves were of great benefit to the working and tradespeople of the suburbs, and increased trade to a considerable extent, forming a connecting link between the trading and manufacturing centres. The objection to them in towns was their interference with other vehicles, a difficulty which an overhead tramway would overcome.

At the invitation of the President, Mr. Conradi then sketched upon the blackboard and explained two alternative methods which he suggested might be adopted for giving the cars greater stability under all conditions of loading.

Mr. Schönheyder said that at present he did not see the advantages of the system, and he also thought it had been anticipated. He did not see its advantages over the simple lines referred to by Mr. Anderson and other speakers. The cost of the tressels must to his mind be greater than a removable railway and sleepers laid down on the flat ground. It might possibly be useful for a permanent railway where posts could be driven into the ground. If some comparisons were given of the cost of the tressels as compared with the removable railways now manufactured so largely, they would give them a better means of judging of the utility of the system. He might say that he thought the disadvantage that would accrue

from the loading of the car more on one side than another had been very much exaggerated. If the car were fully loaded on one side and not on the other, it would not necessarily throw the full weight of the passengers on the side wheels. Even if they could get the full load, the side wheels would only have a similar load to that on the wheels of any other railway, and if the wheels were properly constructed no difficulty would arise. The difficulty he thought would be with the wheels at the top: there would be a friction on their flanges which would be objectionable. He would like to ask whether the side wheels were made to fit the rails rather closely or whether there was a certain space between them, because he could imagine that, as regarded passenger traffic, there must be an awkward working, from the wheels on one side touching the rail and those on the other side not touching it. He agreed with Mr. Anderson that columns at a distance of 40 or even 50 feet would practically divide a street into two narrow ones, and the centre part of the street would be quite useless. He would like also to ask what provision it was intended to make for going round curves: was the outer rail to be elevated above the inner as in ordinary railways, so as to counteract the centrifugal force?

Mr. W. P. Morison said that, as some remarks had been made with regard to the cost of this line as compared with other light lines, it might be interesting to hear the cost of what he believed to be the cheapest line in England constructed to the standard gauge. This was the Golden Valley Railway in the west of England, which constituted a branch of the Great Western Railway, and was constructed to the standard gauge, so as to enable it to carry ordinary trucks in communication with the trunk line. In round figures it had cost 6000*l.* per mile. One or two gentlemen had referred to the light railways that were being supplied by various manufacturers for plantations and for running along roads. In Italy and in South America these lines had been laid at a cost varying from 400*l.* to 1000*l.* per mile. He thought that it could not be maintained that this system would effect a saving of cost as compared with other light lines laid in the ordinary way with two rails upon a steel sleeper on the ground.

Mr. J. W. Wilson, jun., said that unfortunately he had not been able to see the line in operation, and his remarks might therefore be less practical than he should desire. With reference to the friction on the curves, he apprehended it was not intended to run at a very great speed. He was not quite clear as to the

length of the wheel-base, which might affect the flanges of the wheels in sharp curves.

Mr. Behr: 4 feet 3 inches.

Mr. Wilson said that would meet the difficulty, no doubt. His principal interest in the matter was that he thought it might supply the place of a system of single-rail elevated line which the late Mr. J. L. Haddon had designed, and for which he (Mr. Wilson) had assisted him in laying down a course in the Crystal Palace grounds. That was a line suspended upon standards, and was of a satisfactory nature, and pointed to a great success, but it was not carried out, owing to Mr. Haddon's death. No doubt a line which could be kept uniform by simply altering the elevation of the standards upon which it was supported, had several advantages over lines laid upon the surface of the ground. The author in his interesting paper had mentioned that this system might take the place of ordinary street tramways. At the present time, he (Mr. Wilson) was engineer to a tramway scheme which was to connect two great towns with a considerable tract of country between them. If he adopted the Lartigue system he must do one of two things, either have the standards short, thus bringing the rails near to the ground; or at an elevation, as shown upon the diagram, sufficient to clear the ordinary street traffic. It was obvious that the former plan would be inapplicable in almost any town, while in the latter the expense would be greater, and he must confess that, knowing the district through which the tramway in question would have to pass, he could see many places where the lamp-post standards would be quite impracticable; besides which the streets in many instances were not very broad, and he thought there would be a strong objection to carrying an elevated railway close in front of the houses. He would also mention one other point. In the case of carrying passengers, there must be some provision to enable them to alight wherever they wished, and this was a difficulty of which, if the line was elevated, he did not see the solution.

Mr. H. A. Cutler thought that the wind pressure would be an important element with regard to the friction on the guide rail. It appeared to him that the point of suspension was below the centre of gravity of the carriage. This, if the wind were blowing on one side, would tend to throw the carriage over.

Mr. W. N. Colam said it had been stated that in a few instances only had cables been found to be economical for passenger locomotion. He had had considerable experience with cable lines, and he assured the Members that American

and other results proved the contrary, and that cable traction was growing very rapidly in all parts of the world. The elevated way as shown by the author was not at all adapted for London streets. A similar overhead track had been in operation for a long time in New York, and was found to be such an intolerable nuisance that strenuous efforts were being made to supersede the present steam motors by cables and thus partly improve matters. If a city like New York had found such a structure objectionable, he was afraid no city in the world would receive it. As a member of the Society he felt gratified that a new idea of this kind should have been brought before them in the first instance, and soon after its arrival in England.

Mr. Behr said that Mr. Pendred mentioned that he had not included beetroot, peat, and coffee in the applications. Of course he could not include every kind of application. But for carrying beetroot the system had been largely applied in the north of France. The cost of traction was found to be 5*d.* per ton, while on the Decauville system, which was much used in France, it amounted to 1*s.* 4*d.* With regard to the balancing of the cars, General Hutchinson—Railway Inspector for the Board of Trade—had honoured them three times and Major Marindin twice with an inspection. They took the greatest trouble to load the cars unequally, and expressed themselves as perfectly satisfied with the working of the line. He (Mr. Behr) had made extensive experiments, and so convinced was he of the lightness of the friction round the very sharp curves, that he had taken the original side rails out and replaced them with side rails which only weighed 2½ lb. per yard. He had also dispensed with the cross pieces between the side rails, and found that slight structure quite sufficient to stand the slight strain put upon the curves by the engine and train passing over. Theory was one thing, proof another. He had consulted some civil engineers in Westminster who had persuaded him that he must put a very strong iron strut between the two side rails, because there would be great lateral pressure, and their opinion had induced him to put a solid beam 18 inches in width between the two side rails on the bridge, with the result that he had wasted a good deal of timber and iron, as proved by the above alterations. The best evidence of this was that, with a train passing not only hundreds but thousands of times over the line, the paint was hardly worn off the side rails. That was a more conclusive proof than all the arguments in the world.

As to the columns for elevated railways in the suburbs of towns not being stable with a base of 2 feet 6 inches, he admitted

that to be perfectly true. It never was intended they should stop at the level of the street. The foundations would be of any base required below the level of the street, and would not interfere with the roadway.

With regard to the observation made by Mr. Anderson with reference to the framework of the engine, that the weight was carried in such a way that it would be difficult to keep the joints tight, he could only say that though this engine was constructed without springs, and that must produce a trial to which an ordinary engine would never be subjected, they had run it for two months and a half, over rails with very bad joints, continuously from half-past nine A.M. to half-past five P.M.: they had worked with only one engine, and in the two months and a half they had only had one stoppage of two hours in order to tighten the tubes of the boilers. That was the only time they had done anything to the engine, and still it was in perfect working order. With regard to obstruction in the streets of towns, it was perfectly evident that if you make a railway or tramway of any kind (except in a tunnel) you must cause some obstruction. It was only a question of degree. He always understood that the great object was to relieve the traffic on the surface of the street. He believed in practice tramways did not relieve it in the least, they only made it worse. If they went over Westminster Bridge about five or six in the evening, they would find the whole of the roadway on the Surrey side blocked: neither foot passengers nor carriages could pass because the tramways blocked it up completely, and for his own part he would much sooner go down a street with a lamp-post in the middle. It was only a question between two obstructions. Some people might like to cross with their carriages over tramway rails: his taste was the other way. A number of lamp-posts were already placed in our most crowded thoroughfares, marked "Left" and "Right," and he thought they were placed there because they kept the traffic in order. There might be occasionally a driver anxious to press forward, but generally speaking, where there was great traffic, the keeping of two streams was considered the means of passing the greatest number of vehicles in the shortest space of time.

He had been asked whether the rails upon the curves were inclined in the ordinary way or not. He had inclined them in the ordinary way at Westminster because he believed it to be the best system. General Hutchinson and other engineers had thought it might be dispensed with, and that, on a single rail, it was better to keep the centre of gravity perfectly perpendicular over the rail. This was one of the moot points. If the

Board of Trade told them not to incline it they would keep it straight.

With regard to the cost, of course it was a very difficult matter to give a general idea of the cost of a railway. All the speakers seem to have compared his estimate with the cost of the rails only of the cheapest narrow-gauge lines. He maintained that, taking into consideration the whole cost including cuttings, embankments, the provisions made for drainage and the whole paraphernalia, his iron railway was cheaper than any agricultural railway which must follow the ground and therefore have culverts, banks, and cuttings. Another point was that in disturbing the condition of the ground you never knew how much work might have to be done. A cutting estimated to cost 1000*l.* might cost 10.000*l.* or 20,000*l.*, and it was better to avoid the risk.

Mr. Colam had said his (the author's) elevated line would be like the railway in New York. He was sorry to differ. He had travelled over that a great many times. It was a hideous structure with two columns, with practically a platform overhead which made the street dark underneath, and there were two lines of railway passing there on both sides of the street. A man sitting in a train in the Third Avenue was about five feet from the windows of the houses, and could shake hands with the people in the houses if he wished. There was another line not quite so bad, but still more hideous, carried on a single column bracketed at the top, which had as wide a platform as the other, was just as much an obstruction, and took up half the footpath. If the centre of the road were taken with his line he defied anybody to say that the householders would be annoyed by passing carriages.

The observations made as to the effect of wind pressure he did not understand. He always had an idea that the pressure was exercised on the surface of the carriage and had a certain relation to the surface presented to the wind. Ordinary carriages at the surface of the ground presented a much larger wind area. His rail being at the centre of the carriage, the overturning effect of the wind pressure would be less than on a carriage standing on an ordinary railway where the full height of the carriage acted as a lever from the bottom. In answer to several questions, he stated that there was a space of about an eighth of an inch between the side wheels and the rails. He also said he did not think he should follow the advice given by Mr. Conradi. The idea Mr. Conradi had brought forward was not new, and he could show him drawings suggesting his idea of the side rails. He did not see, however, why he should

increase the cost of the side rails to provide against a difficulty which in practice did not exist.

Mr. COLAM wished to explain that the line he had alluded to was not the main line described by Mr. Behr as such a nuisance. It was not an obstruction to the light, and the objection to it on the part of the people of New York was simply that it was a railway running overhead.

Mr. BEHR said the only line he knew was the one he had described. He believed the Americans were not averse to the idea of an overhead line. Mr. Meigs had within the last few months been constructing a single overhead line in Boston at an immense cost, which did exactly what this line proposed to do at a small cost. The Meigs rolling stock cost about fifty times as much as the Lartigue. It had been running in Boston for the last two months, and had been illustrated in the *Graphic* and some of the German papers, and seemed to show that the tendency in America was for elevated railways.

The PRESIDENT said that if any testimony were wanted as to the valuable character of the paper, it was to be found in the equally valuable and animated discussion they had had upon it. He would not detain the meeting by referring to more than one point raised, and that was the question of the obstruction caused by the proposed columns in the streets. He was disposed to think that being placed centrally in streets of suitable width, they would tend to divide the traffic in an effectual manner, and keep each portion, the up and the down, in its proper route. With regard to Mr. Meigs' overhead railway, so far as he understood it, he should certainly give the preference to the Lartigue system now before them.

The Meigs system consisted in the first place of an iron Cornish boiler-shaped car, about 50 feet long and 11 feet diameter, with rounded ends. The rail standards were similar to those proposed by Mr. Behr for the Lartigue elevated system. On the top of the standards were carried two guide rails, placed a few inches apart, into which geared guide wheels, which were placed horizontally under the car. A couple of feet or so below the guide rails were a pair of working rails carried one on each side of the column, with 10 or 12 inches space between them. The running wheels of the cars traversed those rails at an angle of something less than 45 degrees from the vertical. Those wheels were hung on very heavy axles, carried in equally heavy bearings, the whole forming a series of sharp angles with the car, through which some awkward strains would have to be transmitted. It appeared to him, too, from the drawings he had seen, that the flanges of the horizontal wheels would be

very prone to lock in the grooves of the guide rails, and thus induce excessive friction. There was, however, one redeeming feature about the cars, and that was that in their interior fittings all sharp angles and projections were carefully avoided, whilst they were well padded and nicely upholstered, so that in the event of a roll over, the passengers would receive a minimum of damage as regarded cuts and contusions. He certainly gave the preference to the Lartigue system which Mr. Behr had brought under their notice and for which he was of opinion there was a reasonable prospect of success in the future.

November 1*st*, 1886.

PERRY F. NURSEY, President, in the Chair.

LIQUID FUEL.

By Percy F. Tarbutt, A.M.I.C.E.

Although the question of the utilisation of liquid hydrocarbons as fuel is of no recent origin, having been made the subject of inventions and patents for the last half century, the discoveries of new sources of petroleum in all parts of the world, together with the increase of residual oils produced from gasworks, coke ovens, smelting furnaces, and oil refineries, is now bringing it more conspicuously forward than ever. It is very usual to speak of liquid fuel as petroleum fuel, but this is misleading. No doubt when liquid fuel comes into more general use, the largest supply will be derived from the petroleum regions. Still, however, the sources of production are by no means confined to them, as, in every country where gas is manufactured from coal, and where blast-furnaces and coke ovens are in operation, quantities of tar or residual oils from tar distillation are being produced, to say nothing of the enormous quantities of oil-producing shale in this and other countries which may be made available for the production of liquid fuel.

Although petroleum has been known and used to a limited extent from the earliest historical times, being mentioned by Herodotus, who speaks of the oil found at Zante, and by Plutarch, who describes a petroleum conflagration at Ecbatana, it is only in our own times that the illimitable reservoirs of this substance, which are stored in the earth's crust, have been commenced to be drawn upon to any considerable extent. It is less than thirty years ago since the first successful wells were sunk in the United States, from whence are now produced 40,000,000 barrels annually. The rapidity of the development of the petroleum industry on the Caspian Sea has been even more remarkable. The quantity raised in 1863 was only 5400 tons, and this had risen to 950,000 tons in 1882, and could be increased almost indefinitely were the means of transport less restricted. A very graphic description of the

abundant yield of some of the wells in this district is given by Mr. Marvin, in his pamphlet on "The Petroleum Industry of Southern Russia." He says :—

"The first fountain of the newer kind took place in 1873, when a well belonging to Khalify Company startled Baku by spouting to the height of 40 feet. All the efforts of the company to check it were useless, and immense quantities were lost. Since then there have been fifty or sixty such fountains, the mere enumeration and description of which would fill columns. Let us quote a few striking samples:—In 1875 there was one that spouted 2400 tons of oil every twenty-four hours from a depth of 315 feet, and formed in the course of a month four vast lakes of oil. The owners the next year had another fountain which created in 90 days a lake of oil estimated to contain 100,000 tons. In 1877 the firm of Orbelovi Brothers struck oil at 210 feet, which spouted at the rate of from 1600 to 4800 tons per diem. The total quantity of crude petroleum wasted from this eruption was 160,000 tons. In 1879 there was a well on the Mnatsaknoff estate which spouted about 500 tons of oil a day for 120 days—most of it, as usual, running to waste and being lost. During the last few years there has hardly been any period when some well or other has not been spouting on the Saboontchi plateau. The Ararat Well, in 1880, ejected enormous quantities, and continued spouting until, by chance, a neighbouring well tapped the supply and checked the fountain. The two between them expelled to the surface a quarter of a million tons of oil, and then settled down to a quiet life as pumping wells. Each of these now produces most regularly about 100 tons of oil a day. In 1883 occurred the most remarkable fountains of all. Two of these, the Droojba and Nobel's, have been already referred to. The third, belonging to Lianozoff, spouted dry sand 300 feet or 400 feet high three-quarters of an hour before the oil began to flow, and ejected such a terrific blast of gas that for the whole day the Balakhani locality was poisoned by it. Afterwards the oil spouted nearly 200 feet high. This extraordinary height was surpassed a few months later by the Droojba fountain, which at intervals maintained an altitude of 300 feet. Such spouts of oil as this have no parallel in American records, and have provoked widespread interest among scientific men in Europe.

"The Baku people have not yet exploited to any very serious extent their petroleum deposits. Years must elapse before 1000 feet—a common depth in America—will be attained, and at least two or three decades before they touch the cells, 2000 feet below the surface, as has been already done in many cases in Virginia. The present supply of oil is so

enormous at the feeble depth of from 100 feet to 600 feet below the surface, that no inducement to deep sinking exists. Wells are only being bored in the hope that the impending opening up of the European markets will cause a rapid and unprecedented demand for crude oil, or, as in the case of Nobel Brothers, who have fourteen good spouting wells plugged up, simply to keep the staff employed. . . . Baku can furnish apparently inexhaustible quantities of crude petroleum at a few pence per ton. . . . Baku is linked by steam communication with every part of Europe, and all that is required is to develop the resources of that communication to place the oil in most of the lamps of the Continent."

Other countries from which considerable supplies are obtained are Galicia, Burmah, Canada, Peru, Germany, Roumania, Australia, and Japan, whilst within the last twelve months new discoveries have been made in Egypt and France, and in several parts of India. There seems, therefore, little doubt that the distribution of petroleum is as general over the surface of the earth as that of coal, and the development of these resources is surely and steadily increasing as the use of oil for lighting and for fuel extends.

The quality of the crude oil in different localities varies greatly; the Pennsylvania petroleum yields about 75 per cent. of lamp oil and 25 per cent. of heavy oil suitable for fuel, whilst at Baku, on the Caspian, the proportions are reversed, only 25 per cent. being obtainable as lamp oil, and 75 per cent. as residue. The petroleum now being worked at Sibi, in Beloochistan, contains scarcely any lamp oil, and is suitable only for fuel.

The second source of liquid fuel is derived from the distillation of coal. From the gasworks of the United Kingdom alone there is now an annual production of tar amounting to about 115,000,000 gallons, and this is increasing at the rate of from 5 to 7 per cent. per annum. The employment of tar and its products in the arts has been decreasing for some years, and unless some new commercial products in it, or applications of it, are discovered, there seems every prospect of its continuing to decrease. Tar and the green oils and creosotes obtained from the refineries are not substances which can be stored with ease or without expense, and consequently any profitable method of utilising the surplus production is of importance, even in countries where coal is cheap and plentiful, as is the case in England. Other sources of present supply of liquid fuel are coke ovens, shale oil works, and blast-furnaces, in which the adoption of systems for obtaining the bye-products (including creosote oil) is increasing. The author has not yet

had any opportunity of studying the question of the supply which will be obtainable from the immense shale deposits of the south of England; but according to Admiral Selwyn, who has given many years' attention to this subject, a practically inexhaustible supply of cheap oil may be obtained from this source when it is needed.

As regards the conveyance of oil, the barrel is rapidly being superseded by tank railway waggons, tank barges, tank carts, and tank ocean-going vessels, with a great saving in the cost of freight and handling, whilst in the petroleum regions the system of pipe lines has effected an enormous saving in carriage. In the United States this system has reached a very high state of perfection, and some idea of its extent may be derived from the fact that in Northern Pennsylvania alone there are some 21,000 isolated oil wells connected by pipe lines with Philadelphia, New York, and Baltimore. The total length of these oleoducts in the United States is about 9000 miles. Pipe lines are also in use in the Black Sea oil fields and in the Apsheron peninsula for conveying the oil from the wells to the refineries at Baku, but no line has yet been laid from the Caspian to the Black Sea, although the scheme is completed and tenders are being sent in to the Russian government. The line will be about 600 miles in length, running from Baku to Poti or Batoum, and it is estimated to cost about 2,000,000*l*.

Turning from the sources of supply to the relative values of liquid fuel as compared with coal, we find in studying their theoretical evaporative values that the heat-producing constituents of different oils vary almost as much as those of different descriptions of coals, and it is therefore only possible (without going into a labyrinth of figures) to take a standard in either case, which shall approximate as nearly as possible to the average composition of the two fuels. The average composition of English, Welsh and Scotch coal is then about 80 per cent. carbon, 5 per cent. hydrogen, 8 per cent. oxygen, the remaining 7 per cent. being nitrogen, sulphur, and ash, which for the purposes of this comparison may be disregarded. The average of the liquid hydrocarbons available as fuel may be taken at 87 per cent. carbon, 12 per cent. hydrogen, and 1 per cent. oxygen. The total quantity of heat evolved by the combustion of fuel is of course equal to the sum of the heating powers of its elements, except that when oxygen is present a deduction must be made for the equivalent constituent of hydrogen neutralised by it. The heat evolved by the combustion of 1 lb. of carbon is 14,500 English units, and by 1 lb. of hydrogen 62,000 English units, or about $4\frac{1}{3}$ times as much.

Now in the case of coal a great part of the hydrogen is neutralised by the oxygen, being in fact present in the fuel in the form of water, whereas in liquid fuel nearly the whole of the hydrogen is uncombined with oxygen, and is therefore available for combustion; and it is to this fact that its higher theoretical evaporative value is mainly due. The value of the two fuels is then as follows:—

	English Units.	Lbs. of water evaporated per lb. of fuel at and from 212° Fahr.
Average coal	14,000	14·48
Average liquid fuel ..	20,000	20·7

In practice, however, other circumstances in connection with the methods of combustion of the two fuels considerably increase the theoretical difference between them. The hydrocarbons in coal, being in the solid state, absorb a considerable amount of heat in volatilising, and, in the ordinary method of hand stoking, each time fresh coal is required a large excess of air must necessarily be admitted to the furnace, which has the effect of cooling it down at the very moment when the greatest heat is required. Mr. D. K. Clark gives the following description of what occurs in the furnace when freshly stoked:—

"A charge of fresh coal thrown on the furnace in an active state, so far from augmenting the general temperature, becomes at once an absorbent of it, and the source of the volatilisation of the bituminous portion of the coal—in a word, of the generation of the gas. Now, volatilisation is the most cooling process of nature, by reason of the quantity of heat which is directly converted from the sensible to the latent state. So long as any of the bituminous constituents remain to be evolved from any atom or division of the coal, its solid or carbonaceous part remains black, at a comparatively low temperature, and utterly inoperative as a heating body. In other words, the carbonaceous part has to wait its turn for that heat which is essential to its own combustion, and in its own particular way. If its bituminous part be not consumed and turned to account it would have been better had it not existed in the coal, as such heat would in that case have been saved and become available for the business of the furnace. To this circumstance may be attributed the alleged comparatively greater heating properties of coke or anthracite over bituminous coal."

In an ordinary coal furnace the temperature is subjected to frequent and extreme variations. The generation of gas is

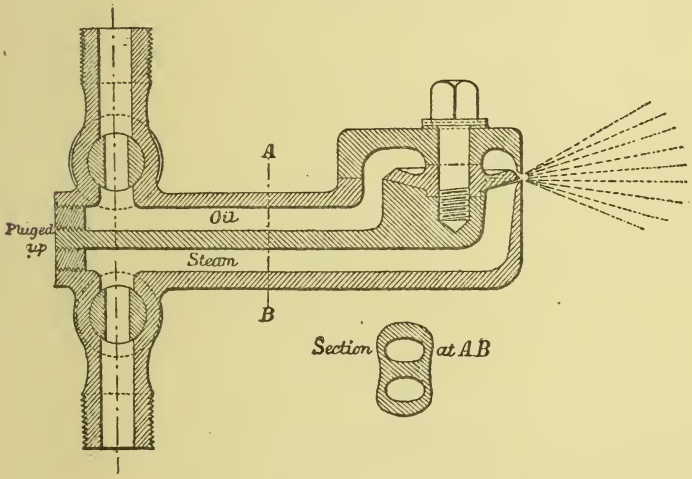
greatest when the furnace is first charged, and therefore at its lowest temperature, and the quantity of air required for the perfect combustion of the fuel (which is constantly varying as the conditions of the charge vary) is not practically ascertainable, nor is it under control. The consequence is that one of two evils is constantly at work—either too much air is being admitted, causing smoke and carrying off both fuel and heat up the chimney, or else there is not enough air passing through the charge to convert the carbon into carbonic acid, resulting in the production of large volumes of carbonic oxide, which escape unconsumed, producing only 4325 units of heat per lb. of carbon instead of 14,500 units, which would be obtained were the combustion of the carbon completed. A proof of the wasteful manner in which coal is generally consumed by steam users was obtained during the investigations made by the corporation of Birmingham about two years ago, when a proposition was before them for laying power mains through their streets. Non-condensing engines of good type work with 2½ lbs. of coal per gross horse-power per hour, but it was found on indicating six engines of this class in Birmingham, taken incidentally, and ranging from 5 horse-power to 30 horse-power, that the average consumption was 18·1 lbs. per horse-power per hour, the highest being 27·5 lbs. and the lowest 9·6 lbs.

Now, in firing with liquid fuel, the supply of fuel to the furnace being constant, and the fuel itself being homogeneous —that is to say, not consisting of volatile and non-volatile portions, as is the case with coal—the supply of air required is constant also, and the right amount is readily ascertainable by observing the chimney and so regulating the air inlets as to allow a thin transparent smoke to be evolved. If there is no smoke, too much air may be passing, and carbonic oxide may be escaping; if, on the other hand, there is not enough air, the thick volumes of smoke given off indicate the fact in an unmistakable manner. With these advantages, it is not surprising to find that when properly consumed liquid fuel yields in practice a higher proportion of its theoretical value than coal. The author has carried out a great number of trials with various classes of liquid fuels in different kinds of furnaces during the last two years, and has now some fifty furnaces working on his system in this country. The system has been successfully applied to marine, locomotive, Cornish, Lancashire, and vertical boilers, with and without cross tubes, and to vertical boilers with Field tubes, to gas retorts, tar stills, steam superheaters, and brass melting furnaces, the average duty performed by the liquid fuel employed being from one and a half to two and a half times that of coal. In one or two trials

with poor oil and in furnaces least adapted to liquid fuel, and where it has been tested against non-bituminous coal (for which the furnaces were specially constructed), the comparison has been unfavourable, whilst in others, where the conditions were reversed, the duty performed by one ton of oil has been as high as that of three tons of coal.

Before describing his own system, the author thinks it may be of interest to glance at the various contrivances which have been devised and patented for burning liquid fuel in the past. In nearly all the systems tried steam has been employed to force the oil into the furnace, and at the same time to atomise or spray it; and the ingenuity of inventors has been devoted chiefly in devising new and improved methods of effecting this object. The simplest form of atomiser consists of a flat, or bats-wing, jet of steam, on to which the oil is allowed to dribble,

Fig. 1.



and is thus broken up and carried into the furnace in the form of a fine spray, where it is burnt, either alone or over a bed of solid fuel. Examples of this plan are the burners of Lents Artemeff and Karapetoff, which have been used in Russia (Fig. 1). They consist essentially of a chamber divided into two partitions by a diaphragm the oil being admitted to the upper division and steam to the lower. Notches are filed in the lip of the diaphragm through which the oil trickles to be blown off by the steam escaping below.

Another form consists of a steam jet placed horizontally and

in such a way as to cut across the path of a stream of oil delivered through a vertical pipe. To this class belong the burners patented by Messrs. Wise, Field and Aydon in 1865 (Fig. 2), and repatented more recently by Mr. Robert Walker,

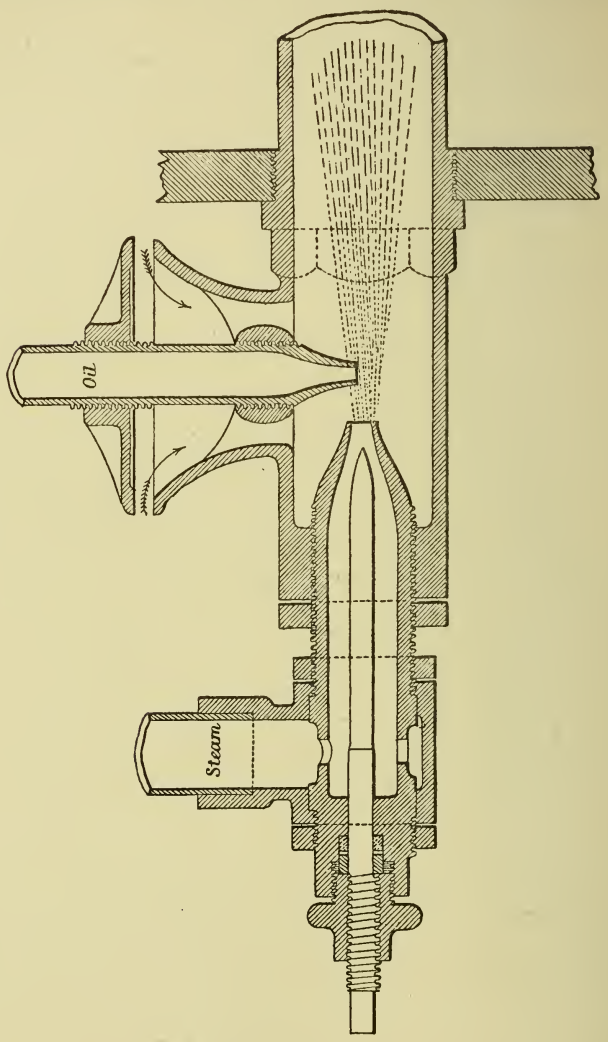


Fig. 2.

also those invented by Messrs. Korting Brothers. Another class of burners consists of an inner jet of steam escaping through the centre of a larger tube to which the oil is admitted, as in

the patent of Mr. Stevens (Fig. 3). These resemble the ordinary water injectors or ejectors. Some of these, as for

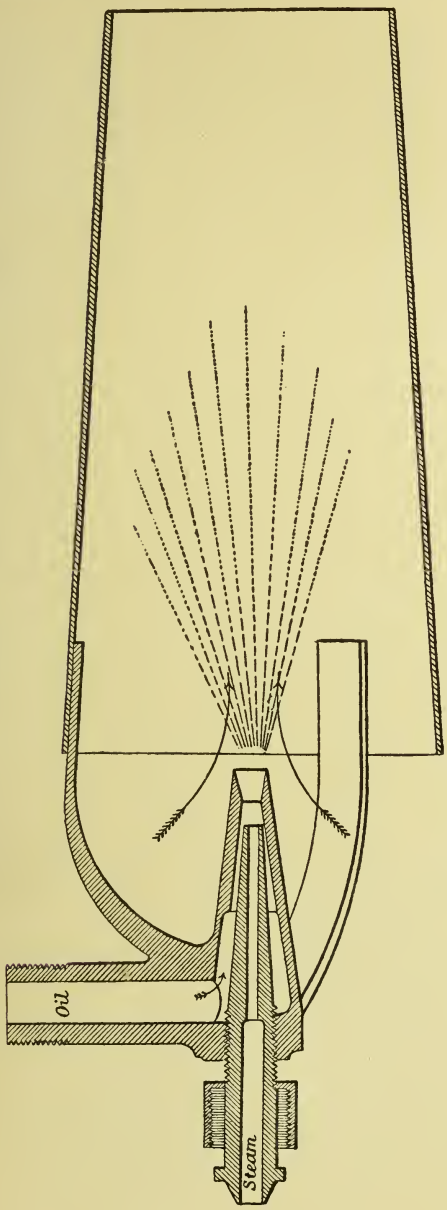


Fig. 3.

instance that patented by Mr. Urquhart, are provided with means for regulating the oil and steam supply in the burner itself, very much in the same way as the water and steam inlets are regulated in the best forms of boiler injectors. Yet another form, and the one which the author generally employs, is the reverse of the last, the oil being admitted through a central jet surrounded by a ring of escaping steam or air. The advantages of this form are that the oil having a clean run through a round hole the danger of its being checked from the choking up of the orifice is reduced to a minimum. There is also greater facility for heating the oil in the burner (as will be described more fully further on), and the spraying is more perfect.

Several inventors have attempted to burn liquid fuel without the assistance of steam or compressed air. As long ago as 1833 Mr. Rutter patented a system of dropping the fuel mixed with water on to a fire of coal or coke, and since then a number of contrivances, such as asbestos wicks, hearths made of sand or other absorbent material, on to which the oil was fed, have been tried without any practical success. Mr. Nordenfelt has recently patented an ingenious arrangement for burning liquid fuel to obtain the intense heat required for making wrought iron castings by the Mitis process. It consists of troughs placed one above the other, so that the overflow from the upper one is conducted to the next in series below. These troughs are placed at the mouth of the furnace, and by means of a strong draught the flame of the oil burning in the troughs is carried to the crucibles containing the metal for the Mitis castings. From a number of experiments the author has tried he is led to believe that this system is not well adapted for boiler furnaces, as with the ordinary draught he has been quite unable to keep steam, and even with a forced draught very large quantities of smoke were produced.

In burning any description of fuel in boiler furnaces it is essential, in order to obtain good results, that the gaseous products should be kept from contact with the boiler plates until combustion is complete. The plates, having water on the other side of them, cannot be raised to a temperature much exceeding 300° Fahr., and since the temperature required by the gases for ignition is very much higher, it is evident that if they come in contact with the plates they will not be consumed. In burning residual oils, which are the most smoky of all fuels, this necessity is most apparent, and various descriptions of furnaces have been devised for the purpose. These have usually taken the form of a more or less modified muffle of fire-brick, into which the oil is injected with sufficient air for its combustion, and

from which the gases are not allowed to escape until they are completely consumed. Furnaces of this form have been the subject of patents taken out by Admiral Selwyn, Mr. Walker, and others. They serve their object so far as the complete combustion of small quantities of oil is concerned, but they are incapable of consuming within the dimensions of the furnace of any ordinary boiler a sufficient quantity of oil to maintain an adequate supply of steam. Moreover, the heat generated within the confined space of the muffle is so intense that no bricks can withstand it for any length of time.

The author has, wherever practicable, adopted the principle of heating by radiation only, which Sir William Siemens found so efficient in gas furnaces. By this means any reasonable quantity of oil may be consumed in furnaces of the ordinary size without the production of any smoke, the steaming capacity of the boiler is considerably increased, and the bricks are not subjected to a destructive temperature. In applying the author's system to a Cornish or Lancashire boiler, the fire bars are removed, and the bottom of the flue is lined with thin bricks (see Plate I.). Pigeon-hole walls A are built from the door along the flue to a distance dependent on the size of the boiler. At the end of the walls is a check plate B, and at various intervals are brick rings C which are in contact with the plates of the flue. On the bottom of the furnace is placed a coil of pipe D, which is covered with thin brick slabs when the furnace is in full work, to protect it from too great heat. One end of this coil is connected to the steam or air supply, and also to the starting apparatus about to be described, and the other end to the outer tube of the burner, the inner tube of which is connected with the oil tank.

In order to escape from the dilemma of not being able to start the fire until steam has been raised, and of not having steam until the fire has been started, a starting apparatus is employed. It consists of a force-pump E and pressure tank F, the latter being connected, through a regulating cock, to the coil in the bottom of the furnace already described. To start the fire, the coil is uncovered and a few handfuls of wood soaked in oil are lighted upon it, and water is forced into the pressure tank by the pump. As soon as the coil is sufficiently heated, the regulating cock G is slightly opened, and water is allowed to trickle into it, where it is at once converted into steam. As this cannot escape back into the tank, it rushes through the burner H. The oil regulator J is then opened, and the oil is carried into the furnace by the steam, where it is at once ignited by the fire on the coil. The oil flame now con-

tinues to keep the coil hot, and by occasionally forcing more water into the tank, in order to maintain the pressure, the fire is kept going until steam is raised in the boiler. This occupies from three-quarters of an hour to an hour and a half, according to the size of the boiler. The starting gear is then shut off, and by means of the cock K steam is admitted from the boiler to the coil, where it is thoroughly superheated before escaping through the burner. The coil can now be covered with the thin brick slabs, and the fire is thenceforth self-feeding so long as the tank is kept supplied with oil.

By varying the thickness of the brick slabs covering the superheater according to the construction of the furnace and the intensity of the heat evolved, the steam may be safely raised to a very high temperature. The advantage of this is that the oil is thoroughly heated in passing through the burner without condensing the steam, and the latter is so greatly expanded that a much less quantity is required for injecting and diffusing the oil. From its position on the floor of the furnace the superheater absorbs heat that would otherwise be practically lost, and hence the expansion of the steam and the heating of the oil is obtained at a minimum cost.

It has been claimed that steam itself, when injected into furnaces in which fuel is being consumed, becomes a source of increased heat—that, in fact, the hydrogen is separated from the oxygen, and is burnt. Such dissociation of the elements of steam undoubtedly does take place under certain circumstances, as in the Siemens generators and other water-gas apparatus. It, however, appears certain that as much heat must be absorbed in separating the hydrogen from the oxygen in the steam as that hydrogen can evolve in recombining with fresh oxygen during combustion to re-form steam; and the author has found in practice that the less steam used in the fire the better are the results obtained. But to whatever extent this using up of the steam from the boiler may be minimised by properly constructed burners, and by high expansion in the superheater, it would still remain a very serious objection to the adoption of liquid fuel on ocean-going steamers for long voyages. Mr. Parker, the chief engineer surveyor of Lloyd's, who took a great personal interest in the trials of the author's system on the steamship *Himalaya*, was the first to point out this difficulty to him. To overcome the difficulty the author has adopted compressed and heated air in place of steam with complete success. The arrangement of furnace and superheater is the same as already described, but the latter is supplied with air from a compressor worked direct from the

main boiler, the steam from the steam cylinder of the compressor being taken back to the condenser. This arrangement works admirably, perfect combustion of the oil being obtained with even greater ease than when steam is employed, and it of course entirely overcomes the difficulty of the salting up of the boilers, as no steam is lost.

The author has not yet adapted this most recent improvement to a steam vessel, but he hopes soon to have the opportunity of doing so, as Her Majesty's government have placed a torpedo boat at his disposal in which to make the trials. The arrangement is, however, frequently employed on a large boiler at his works.

The author would apologise for the scantiness of the scientific data which he is able to lay before the Society. His time has been more occupied in overcoming the various practical difficulties which have presented themselves in adapting liquid fuel to the different types of boiler and other furnaces in the works where his system is employed, than in collecting exact measurements of consumption and evaporation. In one or two instances, however, the steam users have themselves made accurate measurements, and these the author has pleasure in furnishing. At Messrs. Oliver and Company's works at Chesterfield, the system was applied to a Robey boiler, and careful tests were made, under the superintendence of the firm, as to the relative consumption of oil and Midland coal during a two weeks' run. The result was that 1 ton of oil was found to perform the work of 2·6 tons of coal. This boiler was also tested for evaporation by the engineers of the Société General des Diamant du Cap, who, after spending two days in the investigation, obtained a mean evaporation of 16·3 lbs. of water per pound of oil at and from 212° Fahr. The trials made with the apparatus fitted to the steamship *Flora*, at Newcastle, gave an evaporation of 15½ lbs. of water to 1 lb. of oil, as compared to 7½ lbs. with coal. Messrs. Cran and Co., of Leith, are now doing the same work with 1 ton of oil in driving the machinery at their works as formerly required 3 tons of coal. Careful and exact data of the performance of a boiler fitted with this system at Woolwich Arsenal are now being recorded by the government engineers, and the author trusts that when these figures are obtained he may be permitted to add them, together with the results of the torpedo-boat trials, to this paper, which will make it more complete.

DISCUSSION.

Mr. Chris. Anderson said that, having been all his life interested in boilers and engines, he had listened attentively to the paper to ascertain whether he could find three or four practical points in connection with the use of liquid fuel. Speaking on behalf of manufactures of cotton, worsted, and woollen goods, he had failed to see in the paper an indication of anything which would meet their requirements. It was perfectly childish to refer to experiments with engines of five horse-power, as an exemplification of the cheapness of a fuel. Throughout the whole of Lancashire, and in various towns in Yorkshire, there were facilities for testing the value of liquid fuel on a large scale. He was quite certain that the enterprising people who were engaged in the staple trade of those places would be thankful to put at Mr. Tarbutt's disposal boilers equal to 200 indicated horse-power, by means of which a trial could be made upon a commercial footing. Reference had been made to engines at Birmingham consuming 18 lbs. of coal per horse-power per hour. But in these days of engineering perfection it was considered very bad and extravagant practice in Lancashire to consume even 4 lbs. of what was called slack, averaging in price from 5*s.* to 7*s.* a ton. If all the comparisons in the paper were made upon the same basis as the Birmingham engine, that fact would rather detract from the value of the paper. He could give the author the names of half a dozen persons in Lancashire who would give him, free of cost, an opportunity of trying liquid fuel for engines up to 300 horse-power, if he could guarantee a cheaper result than that afforded by the use of coal. In Lancashire, manufacturers were competing "neck and neck," and any engineer who could show a patent fire-bar, or any patent "gimcrack" which would save half a farthing in ten pounds, was held as a desirable person and was not allowed to pass the factory door without finding a customer.

Mr. Edward Field said that it was about 23 or 24 years since he first had to do with the burning of liquid fuel. He was glad to see that the matter was now coming to the front. When his firm brought the subject forward in 1865, the public certainly was not ripe for it, and, besides that, the difficulty of getting oil was very great. So long as they could get it at 2*d.* a gallon, as they did upon first starting, they could work

advantageously as compared with coal; but as soon as the price rose to 3*d.* or 4*d.* a gallon, of course the use of it was stopped. The oil had been a waste product and was simply being thrown away, and it was very great short-sightedness on the part of the producers to raise the price of it as soon as it was applied for fuel on a large scale. Mr. Aydon was the very first to apply the steam jet. Various step-grates, among which was Richardson's, had been tried previously to that at Woolwich Dockyard. The air-jet had also been tried. He certainly believed in the steam-jet, and was of opinion that greater economy was produced by steam than by air, though that was a disputed question. But, no doubt, as Mr. Tarbutt had said, air was extremely convenient, and avoided the necessity of steam for injecting the oil. Still the amount of steam which was required was small, being under 5 per cent. of the water which was evaporated in the boiler. The great object in all the injectors was to prevent the carbonisation of the oil as it went into the furnace. If the oil became carbonised, the injector was stopped, and could not possibly be got clear; and for that reason the oil was taken through a small pipe inside a cylinder surrounded by an annular space through which the steam passed. An injector of such a construction was found to be the best, and it was being used up to the present time. Admiral Selwyn had been using it very successfully of late. As regards the furnace, the introduction of the fire-bricks in Mr. Tarbutt's flue to prevent the heat being abstracted before the gases were fully formed, was undoubtedly advantageous.

Admiral Selwyn, R.N., was delighted to see young engineers of the present day taking up a subject which had occupied his attention for the last twenty years. It had been widely believed that Russia was ahead of England. He could only say that in 1865 the oil was perfectly burned by the apparatus shown in the diagram on the wall as that of Messrs. Wise, Field, and Aydon. He took the late Professor Rankin over to Lambeth to see the apparatus, and the remark of the Professor was, "This is the first time I have ever seen chemical combustion on a large scale." The boiler (Cornish) in that instance was of 25 nominal horse-power. The experiments conducted by him (Admiral Selwyn) at Woolwich in 1868, for the Government, were made with a 150 nominal horse-power marine boiler. The subject of oil burning was introduced to him by Mr. Field, and he suggested to that gentleman the use of an annulus. That appliance had answered admirably up to

the present day. They never had a burner choked, and the same apparatus, with some slight alterations, had been used from 1868. As to evaporation, to say that there was any limit whatever to the quantity of oil which could be used in a given boiler was simply absurd. When coal was used the quantity of fuel was limited by the area of the grate. At Woolwich he had found that he could evaporate one ton of water every ten minutes with about a seventeenth of that weight of oil. He had found that, where steam was used, the duty done was in excess of that which was attributable to the oil. Bunsen, and other experimenters, had found the same thing. However, they did not investigate the reason, but they simply recorded the fact. In experiments which he had made during the last two years he had been running a 40 nominal horse-power marine engine, and for the benefit of engineers he had invited persons to come and make observations of the work. On the recommendation of Mr. Forrest, of the Institution of Civil Engineers, he had invited a chemist of high standing to come and take the gas out of the furnace, while the engine was running, and examine it. The result was to show a composition of carbonic acid gas 18·08 per cent., free oxygen 0·34 per cent., and carbonic oxide 0·34 per cent. There was no marsh gas, no heavy hydro-carbons, and no free hydrogen; but there was 81·24 per cent. of free nitrogen. These figures had been closely verified by repeated trials and showed the most perfect combustion that had yet been known to chemistry. What became of the hydrogen of the steam? Where did it go to? It did not escape. There was only 0·34 free uncombined oxygen, and no free hydrogen; and yet the gas was taken out of the centre of the furnace. He drew the attention of engineers to these facts as not being capable of contradiction, but being capable of verification at any moment. Improvements were in his opinion only being begun. He had seen an evaporation at Victoria Park which showed that something might be done beyond what they had yet accomplished. The shipowners were the principal interest to be thought of in connection with this subject. He had been asked by them to give them twice the effect of coal, and he had given three times. He was delighted to hear Mr. Tarbutt bearing testimony to the fact that about three times the effect of coal had been attained. He had been asked by many persons to fit up steamers for the use of oil fuel; but he had refused to do so because he would not consent to depend upon foreign countries for the fuel of English vessels. He had said that he would not help the shipowners to a conclusion which was as

wrong now as Mr. Field said it was twenty years ago. The oil must be produced at a price which would well pay both the producer and the shipowner. Were those conditions capable of fulfilment? He believed that there was no difficulty about it. The oil could be produced in this country, and put on board ship at a profit, for 21s. a ton, and every ton of that oil would be equal to 3 tons of the best steam coal in actual evaporative power. He had recently made a calculation for a firm of shipowners, and had found that in a vessel with engines of 500 indicated horse-power they could save 1800*l.* a year in short voyages from Antwerp to Marseilles. The owners had informed him that if they could get 500*l.* a year out of the ship, it would pay a good dividend. Land boilers and locomotives would not gain so much as marine boilers by the use of oil. The profitable working of our commercial marine was vital to the commercial supremacy of the country. During the last few months we had seen steamships laid up by the mile, and ship-owning companies, like the Inman, going into liquidation.

He believed that the first person to use jets of any kind for liquid fuel was a certain Major Schapooski in Russia; but his patent was simply a table apparatus, and he had never carried out the operation on a large scale. The first persons, according to his knowledge, who had used the jet in a form in which it was really a success were Messrs. Wise, Field, and Aydon. He believed that the next person was a Mr. Crow. Mr. Crow used one pipe over the other, one carrying steam and the other carrying oil. This apparatus had been in use ever since 1871, at the establishment of Messrs. Johnson and Matthey, the great metallurgists in Hatton Garden. By means of it they were able to melt platinum like lead. The next person who made an improvement was Mr. Donald, of the firm of Donald and Miller, chemical manufacturers, of Glasgow. This firm introduced the fire-brick furnace which was an absolute necessity for the thorough burning of the hydrogen of the steam, which required a temperature of 4000° Fahr.

The question of the production of oil in the country was of the greatest importance. Shale existed in this country in greater quantities than coal had ever been found. It existed right across England in beds 650 feet thick, and in some cases 1100 feet thick, and it lay close to the surface. The development of a sufficient supply of oil to take the place of coal for the commercial marine would, however, be a slow affair. He proposed to turn his whole attention to the obtaining of English oil, so that everybody might rely upon being able to

obtain a sufficient quantity, and until this was done he would not allow his apparatus to be used.

The PRESIDENT said that Admiral Selwyn had said that Messrs. Wise, Field, and Aydon's apparatus was still at work. Were they to understand that it was at work commercially?

Admiral SELWYN replied that he believed that it was working commercially all over the world. It was used in the United States, only the Americans had turned it inside out. He had never used any other injector than that first worked at Woolwich in 1868.

Mr. LLOYD WISE did not think it was clearly understood that the apparatus used at Woolwich in 1868 was on the Wise, Field, and Aydon principle. With that apparatus liquid fuel was projected into the furnace by means of a jet of steam. The boiler was a horizontal one fitted with Field tubes. The experiments at Messrs. Field's in 1865 were made with a Lancashire boiler which was also fitted with Field tubes. At Messrs. Barnes's works, at Hackney, the boiler was either a Lancashire or a Cornish boiler. He believed that they used Crow's modification of the injector. They were chemical manufacturers and produced a large amount of refuse which they were able to burn to advantage by means of the apparatus.

Mr. W. A. MARTIN said that he had had something to do with the oil burning in London in 1863 or 1864. At that time liquid fuel was being used in a large number of furnaces, many having been altered for that purpose. Unfortunately the price of the oil went up, and the furnaces were altered back again to suit coal. When he found that oil could not be obtained in London at such a price as to be cheaper than coal, he withdrew his attention from it; but lately he had had applications from shipowners asking him to see what could be done with oil. They had shown themselves ready to adopt oil, if they could do so successfully. He was not able to tell the shipowners that they could do so, for during the experiments at Woolwich he saw firemen standing by with the water-hose to put out any fire which might accidentally occur. This led him to think that such a fuel was not suitable for use on board ships. He had, however, since then determined to try experiments on the use of liquid fuel, and he had made a jet of his own, such as he had used previously in London. This answered moderately well. He had since obtained a burner from Mr. Stuart of Glasgow which was said to be a very excellent one. It was simply an injector with the steam jet inside. He had made exact experi-

ments both with steam and with air. Air did not answer so well as steam, as it required a strong draught. Using steam, he found that he could evaporate in one hour 1015 lbs. of water with 88 lbs. of oil. In the same time with 108 lbs. of coal he could evaporate 1521 lbs. of water, or half as much again. He could do in forty minutes with coal what it would take an hour to do with oil. When he used oil the temperature was higher in the furnace, but 150° lower in the tubes than when he used coal. His marine boilers were unsuitable for the burning of oil. No doubt oil was a capital thing in a boiler that was made purposely for it, but the ordinary marine boiler was not suitable for it. He believed that they had not got the highest chemical effect from coals, and that they ought to pay attention to that form of fuel because it would take a certain time before oil came into general use. The oil had to be run into the furnace from a tank on a higher level. How could the oil be carried sufficiently high up in a steam vessel? They would have that difficulty and many others to contend with. Another difficulty was that sometimes oil congealed in the pipes and would not run steadily. In consequence of this he had been obliged to discontinue his experiments for some little time. He had found that he could not gain any advantage in his marine boiler by using oil, and believed that with the present arrangements it was impossible for oil to compete with coal.

The PRESIDENT, in reference to the last remark of Admiral Selwyn, observed that he was acquainted with most of the systems in use, and was not aware that the one advocated by the Admiral was one of them.

Admiral SELWYN explained that when he said that Wise, Field, and Aydon's burner was in use all over the world, he meant that the use of steam, in whatever form, to blow in the oil, which was originally devised by those gentlemen, was the practice which was most successful at the present time. If air was used power was required to drive it in; and it had been found that it was better to use the steam direct than to use it to drive in the air. He had omitted to give the exact evaporation in his experiments with a common 40 horse-power marine boiler. It was 5040 lbs. of water per hour evaporated with 230 lbs. of oil; and this was done with "green oil," whose theoretical calorific value, after deductions of two units loss up chimney at 520° Fahr., was only 14·9 lbs. of water vaporisable.

The PRESIDENT could not agree with the statement that the principle of burning hydro-carbons by the aid of an inducing jet

originated with Messrs. Wise, Field, and Aydon, inasmuch as, so long ago as 1830, Pinkus claimed to have used hydrocarbons in conjunction with streams of vapour for steam raising purposes.

Mr. ANDERSON said that intense local heat could not be applied to a Cornish boiler without the boiler bills increasing far more than the saving of the fuel. He hoped that the author of the paper would never attempt to bring about practically the use of oil consumption upon the old lines of the present boilers. They would have the difficulty of priming to get over. It had been said that coal gave only one-third of the intensity of heat yielded by oil; but, even with coal, under forced draught they had difficulty in avoiding priming and knocking out the cylinder ends. Priming had not been taken into account in the paper. He feared that if the supply of liquid fuel for English boilers should get to any great extent into the hands of the American market, it would be subjected to the rigging of their "rings," and the price would alter materially in the course of twenty-four hours, compelling those who used it to calculate the fluctuations of their fuel charges in addition to their regular business calculations, or else to change back their furnaces to the original consumption of coal.

Admiral SELWYN said that there had been an experience of twenty years of the use of oil in every kind of boiler, and in no case had any damage been done to the boilers. The matter was one of constant experience, and not at all one of theory. For ten years the Russians had been running their ships across the Caspian and Black Sea, with oil fuel, and they all said that they would never go back again to coal. They used a much lighter class of oil than we had in England, and the results showed that the oil fuel was only one-fourth the cost of coal, when space and weight saved, as well as other minor consequences, were taken into account.

Mr. C. GANDON said that he had had considerable experience of liquid fuel in burning tar under gas retort furnaces. He had found no particular difficulty in that operation, except that tar required more attention than solid fuel. There was a difficulty in preventing the imperfect combustion of the liquid fuel in gas making; but he apprehended that that would not be so much the case where they had a boiler to deal with, for there would generally be a separate chimney to each boiler, and it would be easy to adjust the supply of air so as to prevent the generation of smoke, whereas, with several furnaces discharging into one shaft, it was at times difficult to ascertain in which smoke was

being formed. In his experience, gas tar gave a result equal to double its weight of ordinary coke or coal, supposing that it was burnt with due care. The question as between solid and liquid fuel depended upon the relative cost of the two. He should prefer solid fuel if it could be obtained as cheaply as the other. The local heat developed by the combustion of liquid fuel in boiler furnaces was very intense; but this difficulty did not so much arise in the case of gas retorts. He had seen creosote and other oils burnt with injectors, and they seemed to answer perfectly well as long as they were cheaper than solid fuel.

Mr. MACKIE said that for about two years past he had seen the experiments which were carried out in London by Admiral Selwyn, and he could confirm what had been said as to the perfect combustion which had been obtained. Rock oil and other liquid fuels required a philosophical mode of stoking. The rough method of throwing on the fuel, which was common in the case of coal, was not suitable for oil. There was no doubt that there was a quantity of shale in this country which would yield rock oil. There was no reason to doubt, either on account of the supply of oil, the perfection of the combustion, or the results obtained, that liquid fuel would be the fuel of the future.

Mr. TARBUTT, replying to the discussion, said, in reference to the question raised by Mr. Anderson as to the experiment at Birmingham, that he should not have stated what he did on that subject without very high authority. His authority was Sir Frederick Bramwell, President of the Institution of Civil Engineers, in his inaugural address in 1885. Anybody who had had experience as steam users would know that a great deal was left to haphazard and to the care, or want of care, of the stokers. Under such circumstances it was not astonishing that results were so very different from what they ought to be. As to the remark that experiments with engines of 5 horse-power were of very little value, he had gone rather beyond that. At one place near London he had had seven double flue Lancashire boilers, from 22 feet to 30 feet in length, working for seven months with liquid fuel. At other places he had had boilers of the same description at work; so that the experiments had gone beyond 5 horse-power. As to the wear and tear of boilers and priming and other objectionable circumstances in evaporating large quantities of water by means of liquid fuel, these were points which it had been most difficult to deal with. If the fuel was burnt in an ordinary

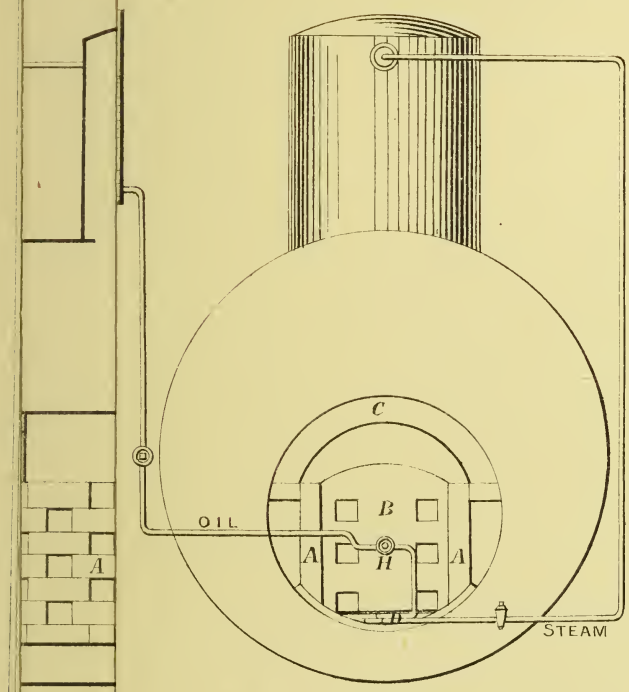
boiler without any lining, it came into contact with the cold plates and went away in smoke, while if the burning fuel came into a close chamber of brickwork there was an intense heat which would burn up anything. In the furnace shown, the heat was no more intense than in any ordinary furnace burning Welsh coal. It was not a close combustion chamber, but was pigeon-holed all through. The only object of the brickwork was to prevent the flame coming into contact with the plates at the side before the whole of the smoke was consumed. He contended that not only they did not lose heating surface by this arrangement, but they actually gained heating surface. The flue was heated as low down as they dared heat it; and the heat, being spread over a large flue surface, was not local and did not endanger the plates. Besides having boilers of considerable size heated with liquid fuel, he had run locomotives with it, and also fired marine boilers of over 800 horse-power. Steam was certainly the most handy way of diffusing oil into the furnace in the state of spray; but unfortunately in the case of sea-going vessels, in which salt water had to be used to replace the steam so employed, there would be a danger of salting up the boilers. It was for that reason that he preferred the use of heated compressed air. He had been delighted with Admiral Selwyn's remarks as to the amount of oil which could be produced from the shale deposits in England. He was sure that Admiral Selwyn must be right in what he stated, as he had given much study to the subject, and that the oil would become available when it was required. He had had some experience in the use of tar for firing gas retorts. At some works the tar could not be sold for more than 3*s.* a ton, and as it would do double the work of the same weight of coal, that would be equivalent to getting coal at 1*s.* 6*d.* a ton, which, he thought, was a desideratum. Mr. Martin had made some remarks which tended to show that the use of liquid fuel was a lamentable failure. He was afraid that if Mr. Martin had failed it must be either the fault of Mr. Stuart's injector, or the way in which Mr. Martin had tried to use it. He did not understand how it was that Mr. Martin succeeded in getting large quantities of smoke. He (Mr. Tarbutt) had failed to do so.

No doubt England owed her greatness to the fact of her iron deposits and her coal deposits being near to one another; but if liquid fuel was coming to the front it was of no use for Englishmen to bury their eyes in the sand, and say that oil was of no use because they had got coal. If shipowners

could not run their ships at remunerative rates with coal, engineers must look beyond the selfish question, and see whether they could not make both ends meet by means of liquid fuel.

The PRESIDENT, in closing the discussion, observed that they had had a very interesting paper and an equally interesting discussion. The question of the introduction of liquid fuel in practice seemed to turn upon the supply of oil. It was well known that there was already a very large quantity of residual products available for fuel in this country. Besides that, steps were being taken to import oil from Russia for use as fuel. And it was not in this country alone that means were being taken to employ liquid fuel. He had before him a lecture on petroleum, by a Mr. John Wallace, which he had just received from India, and in which the author mentioned that the existence of petroleum had been announced at various times in different parts of India. This gave them reason to think that at no distant period that substance would exercise a most important influence upon the arts and manufactures of the empire. The Members had before them the fact that liquid fuel had been successfully used many times; but hitherto the prices went up directly the various apparatus had been got into working order and had reached the practical stage, and its use had to be abandoned. The further fact was before them that the apparatus of Mr. Tarbutt had answered most thoroughly. He (the President) had seen it at work on board the *Himalaya*; and it was within his knowledge that it was at work in a number of boiler furnaces on land, including some at Woolwich Arsenal, where it was doing good work. He did not quite agree with Admiral Selwyn that they ought to wait before using liquid fuel until they could get all the oil from England. The question of the different evaporative powers of fuels had been referred to in connection with coal and with liquid fuel. The same question arose at a Meeting of the Society in December 1884, when a paper was read by Mr. Engert on his boiler, which had given an evaporation of 15·11 lbs. of water per pound of coal. Mr. Schönheyder had tested and reported upon that boiler and had given the evaporation as 13·5 lbs. It was stated at that meeting by Mr. Hartley that the coal which Mr. Schönheyder used in the experiments with the Engert boiler only showed an evaporative power of 12·8 lbs. by the Thompson calorimeter. He (the President) observed upon that occasion that it was so much the worse for the calorimeter, for it was not to be

supposed that fuel would always be burned under the same conditions. Science was progressive, and means would doubtless be found for using coal to increasingly greater advantage. In the Engert boiler the coal was being used under a new set of conditions. So, he took it, it would be with liquid fuel, just as indeed it had been with gas. The more scientifically perfect the conditions the better would be the result, and although theoretically a fuel might only have a given value, yet practically by extraneous aids it might be made to give a higher result than that for which theory would be responsible.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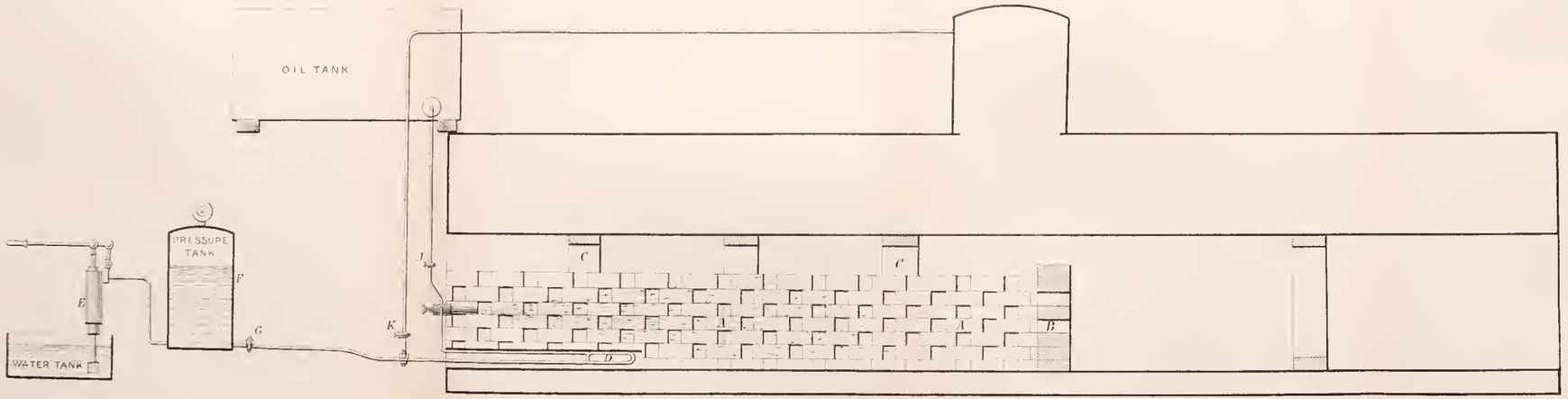
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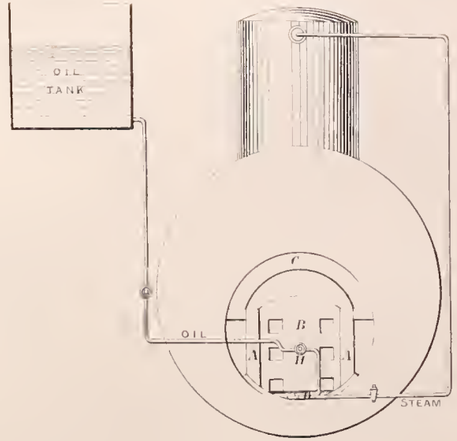
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December 6th, 1886.

PERRY F. NURSEY, PRESIDENT, IN THE CHAIR.

RIVER POLLUTION CAUSED BY SEWAGE DISPOSAL.

By George B. Jerram.

In bringing forward the subject of sewage disposal, the author feels that he may lay himself open to the charge of treading in the steps of many men more experienced and better able to properly treat this important subject; it has, however, happened in the course of his professional engagements that he has had to give special study to the matter during the last five years, both by visiting works in England and reading the numerous reports and papers that have been written and published during that period, as well as by making experiments at the works in his own district. The results of these studies he has endeavoured to carry out in the re-arrangement and extension of the works at Walthamstow, which it is more particularly his intention to describe in this paper.

It cannot be gainsaid that, although the question as to the best means of the disposal of sewage with a view to prevent the pollution of streams (into which formerly the sewage was sent direct) has attracted and occupied the attention of many eminent chemists and engineers for the last quarter of a century, yet the state of our rivers now is much the same if not worse than it was before the Rivers' Pollution Act was passed. So bad is this state, that it must in a very short time draw the serious attention of Parliament, especially if epidemics of fever or cholera should break out in our midst, so that more stringent regulations may be enforced for protecting and maintaining the purity of the streams, which were meant to be sources of health and comfort instead of breeding places of malaria and sickness.

That this is the case has been evinced by the attempts that have been made from time to time to get the law amended by the introduction of various Acts of Parliament.

What form the new legislation will take, whether the proposed

County Boards will be entrusted with special powers for the protection of streams in the several counties under their jurisdiction, or whether powers will be given to Conservancy Boards, composed of representatives from all sanitary authorities situated in the watershed of each individual river, we cannot yet say, but that some amendment in the law is needed, backed up by energetic action on the part of some authority, is a matter on which all will be agreed.

Since considering this question, an important paper, bearing on the subject of the treatment of sewage, has been read before the Society of Arts by Professor Tidy, some of whose conclusions agree with those the author had previously arrived at, although many of his arguments against the separate system, land filtration, and the effect of sewage on land, do not agree with what the author has found in actual practice. Although Professor Tidy has dealt with the various methods of purifying sewage that have been tried, he does not appear to have considered the effect produced on the streams into which the sewage flows. It is quite true that the circumstances and conditions under which sewage should be treated must vary very considerably in different localities. Thus, owing to the proximity of suitable land in one case, broad irrigation or intermittent filtration would be the best means; in another district, owing to the difficulty of getting enough land for irrigation, some form of chemical precipitation should be adopted, with filtration over a smaller area; and in yet another case it would be only possible to treat the sewage by precipitation, thence passing it through artificial filters. But in most cases it will be found that some system of chemical precipitation with filtration over ground that can be cultivated can be adopted. It does not come within the province of this paper, however, to describe the various methods of treating sewage, so much as to draw attention to general facts in connection with the pollution of streams, which exist whatever system is adopted. As an illustration, it may be interesting to record what the author observed on visiting some of the chief works in the country. Thus, *at Leicester*, he found that grave complaints were made by the owners of property and inhabitants of villages and towns below the sewage works, concerning the foul state of the river into which the effluent from these works was delivered. It seems that the lime treatment is the system now used, although experiments have been tried with the A B C process in 1868, and in 1871 the Phosphate of Alumina Company had possession of the works, which they held for eighteen months. In 1877 Mr. Hillé's process was worked for some months; black ash was also tried, and latterly lime alumina, sulphate of iron,

and in addition, the effluent was deodorised by permanganate of soda. In September 1884, the result of the inquiry of the Local Government Board resulted in the decision that the state of the river was too bad to be much longer tolerated. Since then, the Corporation, under the advice of their Borough Engineer, Mr. Gordon, have purchased many acres of land for the purpose of deodorising the sewage by means of irrigation, &c.

At Leeds, where there is a very fine series of tanks, about 600 feet in length, the lime treatment is used; the effluent is strongly impregnated with lime, the result being that the river is very foul some distance from the works.

At Bradford, the process is that of precipitation by lime only filtered through coke breeze. The method of using the chemicals here adopted is varied in an important feature, as instead of the sewage being allowed to flow continuously through the settling beds, it is left, after being treated, to remain in a quiescent state for about one hour before being passed through the filtering medium. Notwithstanding this, the river below the works seems here also to be affected by the influx of the effluent.

At Leamington, where the sewage is used without any chemicals, and passed on to land by irrigation, complaints have been made of the foul state of the river Leam by the towns below the works. This is caused no doubt by carelessness, in allowing more sewage to pass on to the land that it can filter.

At Aylesbury, where the A B C process has been successfully carried on for years, the author found traces of black deposit on the banks of the stream into which the effluent passes.

But to come nearer home. We all know the constant complaints that have been made of the rivers Lea and Thames. The former river is a very good illustration of the author's argument, as, owing to the action of the River Lea Conservancy and their engineer, Major Flowers, nearly all the towns situated on the borders of this river have tried some method of sewage purification.

Taking the chief towns, from the source of the river, the first is Luton.

At Luton, the system adopted is that of chemical precipitation and filtration. Until last year complaints had been made of the state of the river, and in order to remedy this, more land for irrigation purposes has been acquired and new pumping machinery is now being erected for the purpose of conveying the sewage on to the new land.

Lower down the river is the town of Hertford, where sewage

disposal works are constructed, and have been in operation for twenty-five years. A very important action of the Lea Conservancy against the Mayor and Corporation of this town, to restrain them from pouring sewage matter into the river Lea, so as to pollute it, was tried in 1884, and the evidence given at that trial was very interesting, and thoroughly proved the pollution of rivers below sewage disposal works. Much contradictory evidence was given, by equally eminent men on each side, and although the result was in favour of the Corporation, yet it would appear that this was largely due to a strong feeling on the part of the judge that the plaintiff's case was not sufficiently proved. It may not be uninteresting to refer more particularly to this trial, as from evidence given before the Royal Commission on the state of the Thames and Lea, we find that the complaints and descriptions of the kinds of pollution of these rivers are nearly identical.

It seems that repeated complaints were made from the year 1872 that sewage came into the Manifold ditch, by which the effluent from the Hertford works passes into the river, and this, notwithstanding the different forms of chemical treatment for precipitation which have been tried. Things went on until 1882, when there was found to be a large accumulation of mud in the river itself, almost opposite the junction of the Manifold ditch with the river: the description of the pollution is as follows, and is a very fair representation of what is found in all brooks and rivers below sewage works. The evidence quoted is from that given by Dr. Angus Dupré, F.R.S., &c. He states that "the condition of the river Lea was very offensive to the sight, not to the nose but to the sight. There were a great many black masses of fungoid growth floating; large patches of mud on the side, which, when stirred up, were very offensive."

This was corroborated by the engineers, Major Flowers and Mr. Joe Child, who stated that the mud was very foul and similar in character to that found in the river below the Tottenham Sewage Works.

It was admitted that the effluent from the works was clear, and yet this deposit of mud was found in the river. Although the judge examined the river himself and corroborated the statement as to its foul condition, yet Professor Tidy and others visited the place the same day, and stated that the river was in a "state of perfect purity." Professor Wanklyn, Professsor Odling, and others, agreed on the part of the defendants that the effluent from the sewage works did not cause the pollution found by the previous witnesses. The late Dr. Keats, chemist to the Metropolitan Board of Works, stated, concerning samples of water taken in 1875:—"The ditch is in a bad state

and smells almost as badly as ever. This seems to indicate that the treatment by the chemicals used is merely temporary in its effects; the effluent is certainly good and free from any offence, and the works are well and carefully managed." And in 1878 he states, that, "although the ditch was very foul, the effluent water coming from the works is little less pure than drinking water. I cannot explain this, and it appears a very extraordinary state of things."

The judge stated, in his summing up, that the theory that the secondary decomposition and putrefaction of the effluent caused the formation of an artificial mud was left entirely in the region of speculative opinion, as was the idea that this accumulation was caused by the presence of too much chemicals. It will thus be seen how difficult the question appears to be, and how divided the opinions of our most eminent chemists are on the cause of this supposed pollution.

Passing by the works of Enfield and Edmonton, where the sewage is treated over land, we come to Tottenham Sewage Works, where chemical precipitation alone is adopted, and, as is well known, many and constant have been the complaints of the state of the river Lea below these works in late years. It is no doubt unfortunate that the effluent from these works has to pass into a canalised stream, where the flow is exceedingly sluggish, and that no filtering medium is used after precipitation before entering the stream.

From the evidence given before the Select Committee of the House of Commons on the river Lea last session, it is abundantly proved that there is a large accumulation of mud above Lea Bridge.

Great improvements are being made, but although the river has been purer than it was last year, yet the author has found a large quantity of black mud below the sewage works, which, in the hot weather, fermented and caused the death of large quantities of fish. In the lower part of the river, and at its outflow into the Thames, it was in a very foul condition in October last. In a report recently issued by Mr. C. W. Raymond, Inspector of Nuisances for the Poplar Board of Works, he states as follows:—"I have made several inspections of the River Lea, Bow Creek, and streams leading into same, with the result that I found the main waterway in a foul and offensive condition; and this, in my opinion, may be attributed to the following causes:—Discharge of sewage from storm outlet, Metropolitan Board of Works sewer, near Old Ford Lock. Sewage of works abutting on the River Lea, and trade refuse from some of them. Discharge of sewage matter from the West Ham Pumping Station, Bow Creek. Discharge of the

Walthamstow and Leyton effluents. The river above Old Ford Lock was also in a foul state, as evinced by the large quantity of dead fish floating on the surface. In the immediate vicinity of Old Ford Lock, where the storm outlet discharges, there was a large accumulation of sewage matter. At West Ham Pumping Station the sewage is supposed to be treated, and the effluent in a purified state discharged into Bow Creek. This, I am of opinion, is not done effectually, as on each occasion I have inspected I found large quantities of sewage matter on the surface of the water. The water from the works up to Bromley Locks was very dark coloured, and gave rise to a foul and offensive odour, and was a source of general complaint from lightermen and others working at the water-side. When abreast of the West Ham Sewage Works, above the iron bridge, I found the water black, and the surface of it covered with sewage matter and other filth. The stench from the same was very offensive; in fact, the creek at this point could only be compared to a large cesspool."

With regard to the Thames, into which the sewage of the Metropolis has been poured without any deodorisation, after the condemnation of the system by the Report of the late Royal Commission on Metropolitan Sewage Discharge, in which Lord Bramwell and his colleagues say that "the condition of the river is a disgrace to the Metropolis and civilization," there need be no lengthened reference. Since then, under the pressure of strong public feeling, the Metropolitan Board have been considering the question of deodorising the sewage and have decided to construct works at Barking for that purpose, although since 1884 large sums of money have been spent in doing the best that could be done to mitigate the evils complained of. Owing to the kindness of Sir Joseph Bazalgette in sending the author copies of sundry reports made to the Metropolitan Board of Works, he is enabled to give the following short account of the various experiments, and of what is proposed to be done so as to deal permanently with the matter in the future.

In 1884 chloride of lime was at first mixed with the sewage in quantities of from 50 to 60 tons per day. The total quantity of disinfectants used during the twelve weeks ending September 30th, was 2717 tons at a cost of 28,258*l*. Experiments were afterwards made at Crossness on eight million gallons of sewage daily, although the whole of the sewage discharged into the river is deodorised by chemicals. The system of precipitation adopted by the Board in the treatment of the sewage is by means of 3·7 grains of lime in solution, and 1·0 grain of protosulphate of iron per gallon.

The chemicals are arranged to be added to the sewage in the following manner:—Lime water is made in tanks kept thoroughly stirred. The lime water is mixed with the sewage as it passes into the works, and the mixture is kept agitated, after which the iron is added, and the sewage treated is run into tanks where it rests for two hours. The effluent, which is of a brownish colour at times, free from suspended matter, is drawn from the sludge by means of a gully-dam. The sludge is then passed into a sumpt, and thence is forced by compressed air into the lime-mixing tank and afterwards into the press.

The operations connected with the deodorisation of the sewage are completed by the addition of manganate of soda and sulphuric acid. The manganate of soda is ground in mills, through the pans of which a constant stream of water is kept flowing. The solution of manganate of soda flows to the outlet sewer by means of an open conduit, receiving on its way the sulphuric acid, which converts the solution into permanganate of soda. This process of deodorisation is in operation at twenty-three stations within the Metropolitan area in addition to the outfalls. The new works in connection with the northern outfalls, and for the construction of which tenders are now asked, will be arranged so as to treat the sewage in a similar manner.

Notwithstanding the means that are being adopted to deodorise the Metropolitan sewage, there have been complaints this last summer and autumn of the polluted condition of the Thames, both from the districts below the outfalls, and from the Corporation of London. At a meeting of the Common Council in September, it was stated that the river, as far as Purfleet, was in a most deplorable condition, black as ink in many parts, and the stench arising from it was intolerable.

Up to last year similar black mud was present in the brook where the Walthamstow effluent discharged, and has been a constant source of annoyance to the next district through which the stream flowed. It was whilst studying this instance that the author found out the primary cause, the remedy for which he has since tried to discover.

The foregoing illustrations will have proved the prevailing pollution of streams and rivers below sewage works, whatever system of sewage purification is adopted, and that it is more marked when there are not any means of filtration through a sufficient area of land, or where broad irrigation and intermittent filtration have not been adopted. It will be noticed especially that the complaints of annoyance from pollution occur in the summer and autumn months, and then only with

the atmosphere at a certain temperature and lack of rain. The effect of this seems to be that the deposit, directly it is exposed to a certain amount of heat, rapidly decomposes, and the gas which is generated lifts off masses of light decomposed matter, which rise and float on the top of the stream, the gas evolved through the operation causing the smell, which is not only a nuisance but prejudicial to health. There is no doubt that with many chemical precipitates the effluent is affected by exposure to the action of the atmosphere, and secondary decomposition sets in, and this is more observable with some methods of treatment than with others.

The cause of the deposit which is the result of chemical precipitation is, in the author's opinion, very simple. It is well known that in order to obtain a certain result chemically or otherwise, exact proportions of the agents must be used. If more, or less, than is necessary be used, the result cannot be satisfactory.

Now it is a fact not sufficiently considered, that sewage varies in different towns, in some being only simple sewage, while in others the sewage is mixed and highly impregnated with the refuse from dye works, bleach works, and other industries and manufactures. Different chemical treatment, therefore, becomes necessary, and not only so, but in each town the sewage varies in volume and quality, not merely on every day in the week, but also from hour to hour. In suburban districts it is found that the sewage on Monday morning is much stronger and denser than on Friday, owing to the fact that many more are at home on the Sunday; and again, Tuesday being in many places washing-day, the sewage is also of a different nature on that day. This was curiously brought to the author's knowledge by Mr. Hanson, C.E., who was treating the sewage at Aldershot town, where he found that on Sunday the quantity of chemicals he considered necessary for the sewage of a certain population did not properly do its work, and on enquiry, found that this was caused by the influx into the town on Saturday night of some thousands of soldiers from the adjacent military camp.

But it may be said, and has been acted upon, that to secure that all the sewage is treated properly at all times, the proportion of chemicals to be used must be such as will suit the strongest sewage that will have to be dealt with. What is the consequence? At certain times there is a superabundance of chemicals in the effluent, and here, in the author's belief, is the cause of the black deposit (assuming of course that every care is taken to treat the sewage regularly). This effluent, highly charged with chemical, which is a precipitant, acts on the

organic matter, such as decayed vegetable material, manure from off the land, &c., which is present, more or less, in all streams, and according to the speed of the flow, deposits this in the form of black mud, either near or far away from the point where the effluent discharges. This can be experimentally shown by pouring some of the effluent into some water from a stream. It will be seen that a few grains of matter are soon deposited at the bottom, and when it is considered that this is going on day and night, with quantities of effluent varying from half a million to two or three million gallons in the twenty-four hours, it will easily be understood how soon a thin coating of deposit is formed.

The irregular method generally adopted of gauging the quantity of chemicals to be used also greatly accelerates this action, as at one time the effluent will not be sufficiently deodorised, and will contain a preponderating proportion of raw sewage. This is supposed to be corrected in the next hour by the attendant using an excess of chemicals, but the result is that the latter effluent being thus surcharged with chemicals precipitates the excess of sewage left in the former discharge in the brook or river.

Such being the case, the question is how best to remedy the evil? It is quite possible to arrange machinery whereby the quantity of chemicals may be adjusted according to the varying flow of the sewage. This has just been done, among others, by Mr. Hanson, who has invented a machine which is at work at Golcar. It is perfectly automatic in its action, the hoppers containing the chemicals being made to open according to the quantity of sewage; and in the new works at Walthamstow, although there is no head of water by which the Hanson machine could be worked, arrangements have been made for the adoption of the same process, which will be described hereafter.

This mode of regulating the chemicals is quite correct as regards treating the varying quantities of sewage, but as the quality is constantly changing, it is impossible by any mechanical means to regulate the exact quantity of chemicals to the special sewage at a certain time. There seems to be only one effectual method, and that is by passing the treated sewage over land where crops are grown; the effluent of the different periods is there mixed on the ground, and purified by filtration.

Another method would be by having enough reservoir or tank room to hold one day's supply of sewage; thus the whole would be mixed together, and the appropriate quantity of chemical could be applied. The effluent of the previous day would be filtered, and the whole would be even in quality and nature, but this would entail very large and costly works.

With regard to the filtration through land, the author would observe that the Metropolitan Board of Works have abandoned that method of dealing with the sewage, as the following extract from the Report shows:—

"The Royal Commissioners seem to have been of opinion that the only way of attaining complete immunity from smell was, after precipitation of the solid matter, to further purify the liquid by a process of filtration through earth, and they advised that such a process of filtration should be adopted, if it were decided to discharge the sewage effluent into the Thames in the neighbourhood of the present outfalls. The acquisition of sufficient land, however, in the neighbourhood of Barking and Crossness, to enable the vast quantity of London sewage to be effectually filtered through the soil was found upon examination to be attended with such great difficulty, to say nothing of cost, that the Board conceived it to be its duty to endeavour, under competent advice, to find some other method sufficiently effective to obviate the necessity of earth filtration. What was required seemed to be an oxidising agent, which would not only effect the immediate destruction of any offensive odour still remaining after chemical precipitation, but which would at the same time prevent the development of offensive gases. It was found that permanganic acid was effectual in accomplishing both these objects.

"Your Committee were extremely anxious that there should be no mistake in the conclusion arrived at upon this important point, as upon it would mainly depend the determination of the course which the Board should be advised to take. The opinion of four of the most eminent chemists in England (one of them a member of the Royal Commission here referred to) was accordingly obtained by the Board. They all, after careful observation of the experiments made, gave it as their opinion that, if the effluent produced as above by precipitation with lime and proto-sulphate of iron, were subsequently treated with manganate of soda and sulphuric acid, it would be deodorised and purified to such an extent as to render its discharge into the river unobjectionable at all states of the tide.

"With this authoritative opinion before them, your Committee felt that the filtration difficulty might be regarded as overcome; and that the Board might safely conclude that the adoption of the process of precipitation, with the further resort to permanganic acid in hot weather, as proposed, would effectually render the discharge of the sewage into the river innoxious and inoffensive all through the year."

Professor Tidy also agrees that it is possible, by the addition of chemicals to the effluent, to so far purify it as to render it

perfectly innoxious to any stream into which it may enter. But at present this is only experimental, and it seems at any rate a very wasteful process to use expensive agents to kill the manurial properties of the sewage, instead of using the same over large areas of land which are suitable for irrigation, and by which the food supply of the country could be considerably augmented.

As stated at the commencement of this paper, it was the necessity for altering the Sewage Works at Walthamstow, to obtain better purification of the sewage, that led the author to examine this subject, and to form conclusions thereon. He therefore proposes to give a description of the works as executed, and the result of nine months working especially during the hot weather in the summer.

Walthamstow comprises a district of over 4500 acres, and a few years ago consisted only of large mansions and parks with a few small villages or hamlets, but during the last few years it has developed into a large and increasing suburb. In 1871 the population was about 11,000 and is now estimated at about 34,000.

The district is being rapidly covered with workmen's buildings; the old mansions having been pulled down, and the ground on which they were erected with the surrounding land, sold for very high prices, and cut up into streets. The length of public streets is over 50 miles. The dry weather flow of sewage is about one million gallons in 24 hours, and the wet weather about two millions.

As the place has grown, so the necessity for providing a proper system of drainage has arisen, the sewage originally passing into a brook called the Dagenham Brook, which is under the control of a special authority, constituted in olden times, and known as the Dagenham Commissioners, whose duty it is to see that the water in the brook is pure enough for cattle to drink. The brook flows through Walthamstow, passes thence through Leyton, and discharges into one of the branches of the river Lea. Before the East London Water Company's works were constructed, the lower portion of the district was subjected to periodical floods, and the brook contained a good flow of water; but a great deal of this was diverted therefrom for the purposes of the water company. Complaints were made concerning the pollution of this brook, both from the adjacent district below, and by the Dagenham Commissioners, which resulted in a perpetual injunction being obtained from the Court of Chancery against the Walthamstow Authorities to prevent any sewage or foul water passing into the said brook.

In 1877, the Low Hall Farm was bought, comprising about

160 acres of freehold land, situated at nearly the lowest part of the district, at a cost of 26,000*l*. A general scheme of main drainage was designed by Messrs. Grindle and Swan, and soon after executed. The duplicate system of sewerage was adopted, but the main sewers were constructed at that time.

The sewage from a population of about 18,000 had then to be dealt with. The works for sewage disposal consisted at that time of three precipitation tanks, each 150 feet long by 30 feet wide and 6 feet deep.

A small wooden mixing shed comprised the whole of the buildings. The sewage, after treatment with lime (by hand labour only), passed through the precipitating tanks into a brick channel elevated above the adjacent land. This is about one-third of a mile in length, having terra-cotta sluice valves, with slides of the same material inserted at intervals. About 60 acres of the land was levelled and sub-drained. Owing to the low position of the ground, the pipes had to be laid at a depth of only 4 feet. The sludge was pushed by manual labour into a channel, and thence into brick tanks, from which the water was drained into the brook. This, however, was soon seen to affect the brook, and all the water off the sludge was then pumped by hand power into the precipitating tanks. The farm was let to a tenant who covenanted to take all the sewage, and so deal with it that it should not affect the stream, and also to take the sludge.

This was the state of the works when they came under the author's supervision. After some time it was found that the interests of the Board and those of the tenant clashed; the effluent was not satisfactory, owing to the sewage being allowed to flow on to the same portion of land for too long a time. Complaints were still made from the adjoining authorities as to the continued pollution of the brook, and much annoyance and expense were incurred by the then existing state of things. Amongst other reasons for the bad state of the brook, it was found that the pipes for underdraining had been covered with loose gravelly soil instead of clay, the consequence being that in the dry weather, the top soil, which was loamy clay, cracked, and the effluent went into the pipes without being filtered through the land. The lime was only mixed by hand labour, and supplied to the sewage as the attendant might judge expedient. Experiments were made by patentees for using other chemicals, but the result was still unsatisfactory. Great prejudice existed in the minds of some of the governing body against the use of steam, as owing to the position of the works it was considered that it would be unwise to make them too conspicuous.

At last, the question of using machinery was rendered com-

pulsory, from the fact that an order was made by the Local Government Board to provide drainage for the low lying portion of the district, which necessitated the erection of pumping machinery. The whole question of completing the surface water and soil drainage of the district was consequently placed in the author's hands, and the works, as described hereafter, were erected. In order to provide for the drainage from the low level district, a main sewer 18 in. in diameter was constructed; the ground in which it was laid was in water, the level being some six feet below the bottom of the adjacent brook, rendering it necessary to use pipes of a special character to ensure their being perfectly watertight. The pipes used, and which are a great success, are those known as Hassall's patent, consisting of an improved double Stanford joint, between the rings of which cement is poured. This sewer discharged into an underground tank capable of holding two days' supply of sewage. The tanks had to be placed in soil which was full of water; the walls and bottom are built of Portland cement concrete, and the tank covered over with iron girders resting on iron stanchions and concrete arches. Pipes are laid outside the walls on two sides, and arrangements made so that the sewage can pass direct to the pump well without flowing into the tank. It was arranged to pump the sludge from the precipitating tanks into troughs erected at sufficient elevation to ensure it passing on to the land by gravitation. Thirty-five more acres of land, making 100 in all, were sub-drained and prepared for sewage, and the drains so laid that the water must pass through the greatest depths available into the bottom of the pipes. Manholes were constructed at the junctions of the mains, supplied with sluices, so that the pipes could be easily cleansed if necessity should arise. A new carrier was constructed of Portland cement concrete, 6 inches thick, raised above the land on a bank about a mile in length, at a gradient of 1 in 5280. It is so arranged that now the whole of the land can be irrigated when more land is needed for the increased quantity of sewage.

The sewage and sludge are lifted by means of a pair of 10-inch direct-acting steam pumps, constructed by Messrs. Hayward, Tyler & Co., and specially adapted to take thick sludge (Fig. 1). These are so arranged that either pump is capable of taking the sewage from the low level direct into the sewer at the back of the screens, or sludge from the sumpt to the elevated troughs, the sludge being conveyed to the pump-well by means of a culvert laid at a gradient of 1 in 40, connected with each of the three precipitating tanks by sluices. Steam is generated in two 12 horse-power locomotive tubular

boilers, each boiler being capable of supplying enough steam to work both pumps if necessary at the same time, and each fitted up with donkey pumps and feed water heaters.

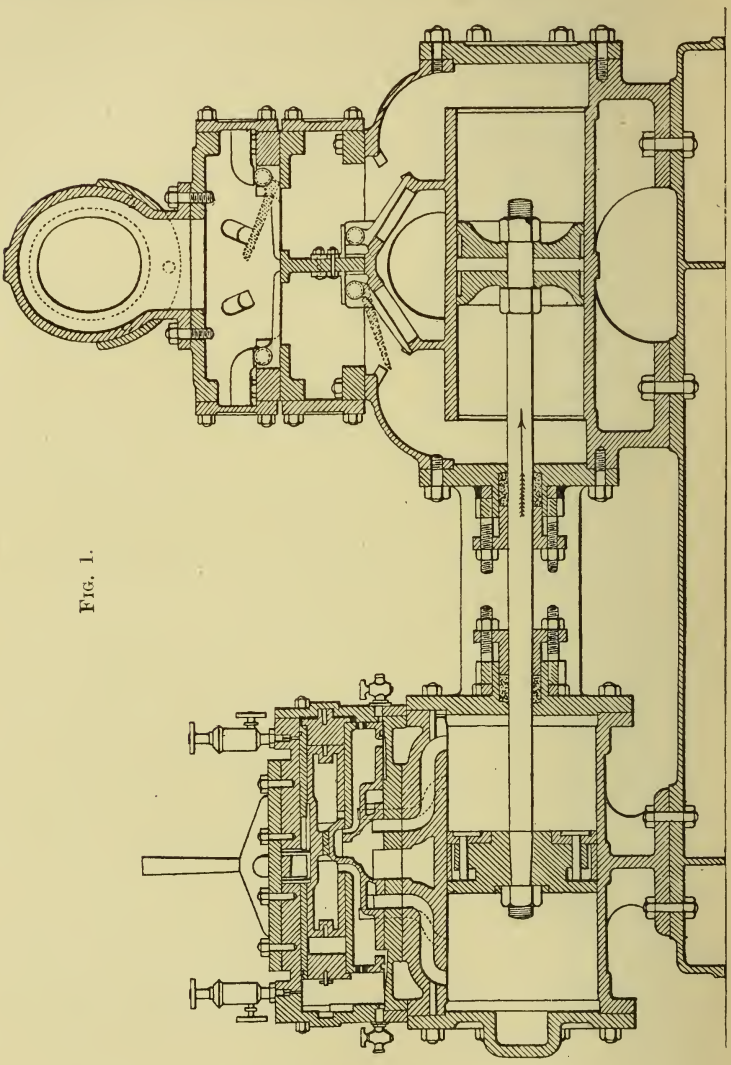


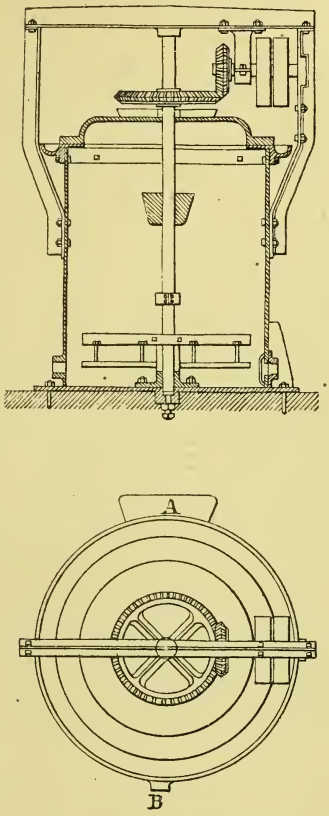
Fig. 1.

The mixing machinery is placed in a room at some little distance from the engine and boiler-house, between which are the testing and store rooms. The motive power for this machi-

nery is supplied by a 6 horse-power horizontal engine, made by Messrs. Tangye and Co.

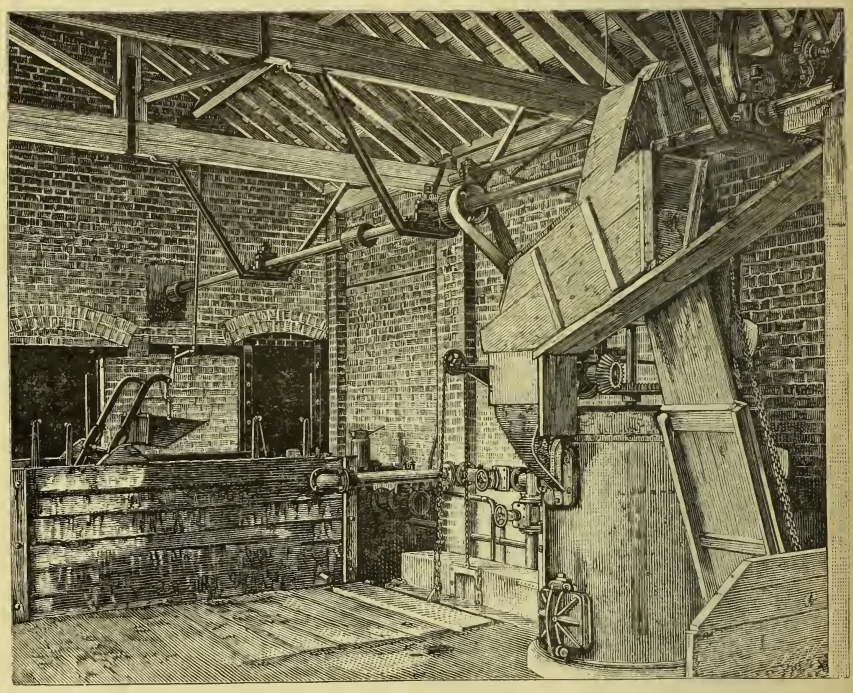
The machinery comprises a 200 gallon lime-mixer, supplied by Messrs. Bowes, Scott, and Read (Fig. 2), consisting of a vertical cast iron cylinder, in which is placed a shaft with movable arms—the cover revolves with this shaft, and is made to work on the edge of the cylinder fitted perfectly true; outside this is a hollowed rim, the action being as follows: the chemicals, which at present consist of lime, salt, and carbolic acid, in the proportion of 1 cwt. lime, 7 lbs. salt, and ½ pint of carbolic, are raised from the ground by means of endless chains and buckets, and pass into the lime-mixer by the inlet A. Sewage is pumped into the mixer instead of water, by a 3-inch direct-acting steam pump, and the whole is thoroughly incorporated by means of the movable shaft and arms, and passes upwards between the cover and cylinder (where all is finely ground) into the hollow rim, and from thence by the outlet B into the main body of sewage in the sewer underneath, as it comes from the town. It is here received into a small iron tank fitted with a sluice perfectly true, so as to reduce friction. This sluice is suspended by a chain passing over a pulley to the end of a lever worked on a small shaft; the lower end of this lever is fixed to a large copper float which is acted on by the varied flow of the sewage, and the whole so adjusted, that as the sewage increases or decreases in volume and velocity, the sluice of the tank is so opened as to let out the proportionate quantity of chemicals, according to the increased or decreased flow of the sewage, so that the sewage itself regulates the proper supply of chemicals.

Fig. 2.



In order that the sewage may be deodorised regularly and evenly at night, without the necessity of anyone working or being in attendance, a large tank is supplied with a strong solution of the precipitating agents; but as this solution would precipitate itself to the bottom of the tank, it is supplied with an automatic apparatus to keep it in a state of agitation all night. This consists of a perforated false bottom under which is a perforated pipe supplied with water; the water passes upwards through the holes and keeps the mixture soft. On the

Fig. 3.



top of this plate is placed a frame carrying five rows of brooms, to one end of which is fixed a chain working over a pulley at one end of the tank, and fastened to a heavy weight. The other end of the frame is hung on to two levers, which work on a shaft at the top of the tank; to the top end of these levers is suspended an iron self-tipping trough; this frame is moved by the action caused by a continuous flow of water from a half-inch pipe into the tipping-trough, and the whole is so balanced and adjusted, that before the tipping-trough is full, it becomes heavy

enough to push the frame forward, and when the trough is quite full, it empties itself, and the suspended weight pulls the frame back so that the whole contents of the tank is agitated both by the movement of the brooms at the bottom, and the sudden influx of the water from the tipping-trough. This machine, which is shown in Fig. 3, has acted every night for over eight months in a very successful manner. The solution then passes into the tank over the sewer and the requisite supply of chemicals is controlled by the sewage. By this means uniform proportion is attained in supplying the proper quantity of chemicals to the varying flow of sewage at *all* times. The treated sewage, after flowing into the tanks, is conveyed on to the land, and care is taken to change the point of discharge, so that it shall not flow on to the same portion of land more than twenty-four hours at one time. The soil of the farm is sand and gravel; the quantity of sewage is about four million gallons in twenty-four hours.

In order to test the purity of the effluent, samples were taken in dry, hot weather as follows: samples of the sewage proper, of the treated sewage as it passes on to the land, and of the effluent as it flows into the brook, were taken every hour during twenty-four hours, the several samples of the respective kinds were mixed together to obtain an average and sent to the county analyst for analysis, with the following result:—

REPORT ON THE WALTHAMSTOW SEWAGE ANALYSED BY THOMAS A. POOLEY, ESQ., B.Sc., F.I.C., F.C.S.

	A. Ordinary sewage.	B. Treated sewage as it passes on to the land.	C. Effluent as it flows into the brook.
	Grains per imperial gallon.	Grains per imperial gallon.	Grains per imperial gallon.
Suspended matter { Mineral	8·05	1·54	1·40
Suspended matter { Organic	20·54	2·52	0·84
Total solids in solution	58·80	45·50	49·00
Chlorine	8·05	4·76	6·11
Free ammonia	4·06	2·54	1·05
Albumenoid ammonia	0·42	0·21	0·14

I consider the effluent "C" lately analysed by me is very good and unobjectionable. (*Signed*) THOMAS A. POOLEY.

The cost of chemicals taken from actual figures over six months averages 16*s.* per million gallons, and the total cost of labour, including pumping the sewage from the low level district and the sludge on to the land, averages 30*s.* per million gallons.

The farm is managed by a farm bailiff under a Farm and Sewage Committee, presided over by a practical and successful farmer. This season six crops of rye grass have been cut and the crop of mangold wurzel averages from 40 to 50 tons per acre. There is a good demand for all the produce, and the result this year is that the farm just produces 4*l.* per acre after paying all expenses. This of course does not include cost of pumping and deodorisation. The disposal of the sludge is slightly connected with this question, but there is no need to describe the methods more generally adopted where land is not available, viz., that of pressing or of being burnt in a destructor.

At the works just described, the sludge is passed for a month on to one acre of land, and a fresh acre is used each month and it is found at the end of three years this land is again available for the sludge, the sludge as it dries is ploughed in and the land cropped.

Experiments have been made with a new machine for dealing with the sludge which appears to be an improvement on the method of pressing. It is invented by Mr. Astrop, and the process is as follows:—The sludge is passed into a tank in which are finely perforated rollers constantly revolving, each roller being connected with a steam exhauster which draws off about 50 per cent. of the water. It then passes on to a fine wire web moving slowly, under which are placed vacuum boxes by which more of the water is extracted. The sludge then being in a state of paste, is broken up and drops by means of a trough into a large revolving wire cage on the ground floor. At the upper end of this is fixed a Blackman's fan, drawing 20,000 cubic feet of hot air per minute, and through this cage the sludge is thus dried and passes in the form of powder into a trough underneath, from whence it is conveyed by means of a revolving worm and buckets into sacks. The whole process only takes three quarters of an hour to thus dry the sludge, the special feature being, that by drying quickly the manurial properties of the sludge are more fully maintained than by any other process.

In conclusion, it seems to the author that one great reason of the partial non-success of sewage purification is that the chemist and engineer have not worked together. What is required for a successful issue is, that the chemist having decided what materials to use for precipitation and the necessary proportions, the engineer should so arrange the works that as far as possible the required proportion should be regularly maintained notwithstanding the varied quantity and quality of the sewage.

DISCUSSION.

Professor Henry Robinson said, that he should be glad if the author would supplement his paper by giving some figures as to the cost of the new system of drying the sludge. A vast number of apparatus had been invented for drying sludge, and one of these, invented many years ago, which he had to investigate at the time, involved the vacuum system and failed, as the great cost of carrying out the drying was fatal to the adoption of the process. From some points of view he should have preferred the author confining his paper to a description of the works he has carried out at Walthamstow, for the question of the pollution of rivers raised too many points to be dealt with at such a meeting as that. The author had somewhat laboured the point, as if there was a doubt about it. He (Professor Robinson) thought they would all agree that the Rivers Pollution Prevention Act of 1875 required to be amended. The Bill that was laid before Parliament last year brought the matter to a very close issue, and there was every reason to believe that it would have been carried, had it not been for the disturbed state of party matters, and the necessity for sacrificing a large number of "innocents." He believed that a similar Bill was being matured for next session. He did not approve of rolling up the County Boards question with that of the Rivers Pollution question, as had been done in this paper. The two things were now distinctly separate, and if they were again mixed together, the opponents of the amendment Bill would prevail. All sanitarians agreed that the Rivers Pollution Prevention Act could be amended by making it compulsory instead of permissive, so that it would no longer be permitted for a town or a manufacturer to pollute a river. The trouble of enforcing the Act arose from the fact that the local authorities were often reluctant to move, or to incur expense, more than from any other cause, and they were unwilling even to recognise the fact that polluting a stream with sewage was an offence against public decency, as well as injurious to the health of the community. The case which was sustained three or four years ago by Mr. Bateman, at Farnham, was one which could be referred to by anybody interested in enforcing the present Act. The matter was fought out successfully before the county court judge, and the Act was put into operation, which resulted in the town of Farnham being now drained. This showed that the present Act, though defective, could be administered, if there was a will to administer it. What was wanted was simpler machinery

for enforcing a simpler Act, the fundamental alteration being the making of the Act compulsory. He thought that the construction of artificial filters might have been referred to by the author, as one of the methods introduced of late years for purifying sewage, where large areas of land could not be obtained, or where the land was unsuitable through being of a clayey nature. A filter constructed of layers of alluvial soil and layers of burnt clay would purify large volumes of sewage effectually. It was obvious that if sewage works were carried out badly they would yield bad results. The author stated in his paper that the object at Walthamstow was to pass the effluent over portions of land at different times, or intermittently, to prevent what had happened at other works through discharging on a particular area for too long a time. But if this could be done at Walthamstow, why was it not carried out at other works where similar skill could be obtained? If it could not be, he was sure that Mr. Jerram ought to be retained to do it all over the country.

The same remark applied to chemical treatment. He had over and over again seen works where, under skilful management, the varying flows and qualities of sewage could be dealt with without difficulty, and if that could be done why should means be taken to do automatically that which, after all, could not be done automatically? There must be intelligent supervision exercised at such works, and there must be an intelligent attendant who could apply to the sewage at any particular time the materials which were required for its purification during that period. The automatic machine described by the author was interesting as a mechanical contrivance, and he was sure that the description would be of value to members who were interested in sewage works. Reference had been made in the paper to the great Hertford fight. He (Professor Robinson) had been engaged in the case all the way through, and he did not think that any good result could be brought about by picking out short extracts from the case and stating what A said, or what B said, or what the judge said. He did not think that anybody could succeed in stating the conclusions which were arrived at in that trial in the course of a few observations such as were made in the paper. He did not agree with the conclusions which Mr. Jerram drew from that case, but his reasons would involve too lengthy an explanation for such an occasion as the present. With regard to Tottenham and the other places which polluted the river Lea, the pollution was due to bad works and bad systems, and it was not right to say that chemical treatment of sewage had failed, when the works were defective in size, in arrangement, and in manipulation. Insuffi-

cient accommodation and cheeseparing, to save the ratepayers' pockets on the part of the local authorities, were really at the root of half the bad results that were so loudly attributed to other causes. The consequence of such a policy was litigation, in which more money was spent than would have sufficed to establish proper works at the outset.

The author had referred to the black ash system of treating sewage. He (Professor Robinson) might perhaps ask the author to supplement his paper with a description in detail of the black ash treatment. He knew it perfectly well himself, but there might be gentlemen in the room who were not acquainted with it. It would be as well also to give details of the composition of the sludge and the manurial results which were brought about by its utilisation.

Mr. JERRAM said that the inventor of the system, Mr. Astrop, was present, and would be able to give the information better than he could do.

Mr. F. ASTROP said that the cost was from 8*s.* to 11*s.* a ton. The machine would do about 100 tons in twelve hours, and with a little alteration it could be made to do the same quantity in ten hours. The cost of fuel for that quantity was one chaldron of coke at 11*s.*, and three men whose wages averaged 5*s.* a day each. This would produce five tons of dry stuff.

Professor ROBINSON asked what proportion of moisture the sludge contained when Mr. Astrop called it dry.

Mr. ASTROP said that when it came to his machine, it contained about 95 per cent. of moisture, and that when it was dry it did not contain more than 6 per cent. From 100 tons of wet solid he extracted five tons of dry, like the sample exhibited in the bottle. That sample contained about 5 or 6 per cent. of moisture, and not more, and he thought that they would find as much as that in their morning newspaper.

Mr. W. C. SILLAR said that the great difficulty which had come under his notice for the last twenty years was the process of drying. The trouble and expense incurred in this matter had been almost heart-breaking. At last they had got the filter press, and this they believed to be the best process. Mr. Robinson had added a still later improvement and, he believed, had taken out a patent for it. These were the presses used at Coventry, and were sometimes known as Johnson's presses. They did not, however, produce a dryness of less than 40 per cent. of moisture, but the material was rendered sufficiently dry to be portable. At Aylesbury, after pressing, artificial heat was used, and then the material was stacked in a heap, where it dried to about 14 per cent. of moisture, in which condition it was sold to the farmers. He was rather surprised to hear

Mr. Jerram say that the works at Aylesbury caused a black deposit in the river, for singularly enough when Mr. Jerram visited Aylesbury he expressed his satisfaction with the effluent water, and said he had not seen the river. He was not, however, surprised if a black deposit was formed, for it was not to be supposed that the effluent water went into a pure river. Someone had said in a joke that the conductors of the works at Aylesbury ought to get an injunction against the river for polluting their effluent water. The persons who had charge of the treatment of the sewage did not know at one moment what was coming down the next, and therefore they had to use a maximum of chemicals to avoid the danger of not using sufficient, and so allowing dirty water to get into the stream. The material used was a precipitant, and it stood to reason that if there was any of it left uncombined in the effluent it would act upon the water of the river itself and, if that water were impure, it would send down a precipitate in the river-bed. Therefore he did not think that the fact of the effluent throwing down a further precipitate in a dirty river was really an objection to the efficacy of the process. He was much interested in the model of the self-agitating tank, and he should like to hear from Mr. Jerram whether the automatic arrangement would do away with the necessity of supervision at night.

Mr. JERRAM said that the automatic arrangement had done so for the last eight months.

Mr. SILLAR said that automatic machines were very good if the sewage was of homogeneous quality, but such was not the case, for sewage varied more in quality than it did in quantity, and the amount of chemicals which ought to be used depended chiefly upon the quality of the sewage. In a report of Mr. Jerram's visit to Aylesbury, Mr. Page had said that he did not think an automatic arrangement would work satisfactorily, and that he had a very excellent automatic arrangement in the form of a British labourer who was standing there in his shirt sleeves. He (Mr. Sillar) had never been able to improve upon such an automatic machine as the labourer. The human automatic machine had answered so well at Aylesbury, that though they had six acres of ground expressly for the purpose of allowing the sewage to be spread on land if requisite, it had never been thought requisite on any occasion to use the ground for that purpose. He had not a word to say against the extra purification of sewage, for if he knew that sewage was going to be turned into a stream, the water of which would be used for drinking purposes, he should not be satisfied, as a mater of sentiment, unless the sewage first passed through land. The action of the earth upon the sewage was not simply that of mechanical

filtration. It had a chemical action, and was a laboratory as well as a filter. It had been shown clearly that if, instead of taking the sewage to the earth, the earth was taken and put into the sewage, the same kind of chemical action would be exerted, and the effect be precisely the same, and there was no question that this was the more economical way of proceeding. When the earth was so used the result was a very valuable agricultural product. Recently Dr. Edmunds, speaking as a medical officer of health at the Society of Arts, said that too much stress was laid upon matters in solution in comparison with matters in suspension. So far as the nuisance went, the dissolved matters were the most offensive, but in respect to health, he said that the suspended matters were of the most consequence, for these contained all the germs of organic disease. Therefore great attention should be paid to obtaining perfect purity from suspended matter. He wished to ask Mr. Jerram how he could account for the chlorine in his sewage being reduced from 8 to 4 per cent. before it went into the land, and again increased to 6 per cent. after it came out of the land.

Dr. Arthur Angell said that he had listened to the paper with very great interest, and in many respects he agreed with it. He gathered that the author would agree with what Professor Robinson had said—that the chemist and the engineer were two necessary factors in the treatment of sewage. He (Dr. Angell) would go so far as to say he believed that the first person to be called in for the treatment of sewage was the chemist. In some cases, however, the chemist found the sewage works already constructed, and he then had to adjust the chemistry to the works. He agreed with Mr. Jerram's conclusions, that in order to get the highest and best possible results, precipitation should be used in connection with some kind of filtration. Irrigation was a very good form of filtration if there was sufficient land of suitable quality for the purpose. It was more expensive to obtain a result of absolute or nearly absolute chemical purity than to obtain a reasonably and fairly practical result in a cheap way and then to filter the effluent. There were many ways in which the chemist could purify sewage, provided that he had an unlimited purse; but speaking practically, an effluent could be produced which was sufficiently pure to be turned into bodies of water of sufficient size compared with the bulk of the sewage. He was not, however, prepared to say that an effluent of sufficient purity could be produced by precipitation alone, at a practical cost, in cases in which the bulk of the effluent was largely in excess of the stream into which it was poured. As to the accumulation of black flocculent matter which occurred in certain streams

below sewage works, it had been hinted in the paper that it was due to an excessive use of chemicals. He agreed with the remarks made by Mr. Sillar on that point. It was evident to his mind, as a biologist and a chemist, that it was a want of chemicals rather than an excess. If putrefactive changes took place in the effluent, sulphuretted hydrogen would be given off, and if, as in nine cases out of ten, there was iron in solution in the stream, black sulphide of iron would be formed and deposited on the vegetable matter. He believed that was the true explanation. It gave him great pleasure to hear Professor Robinson refer to the Farnham case, and say that he considered it a typical one, inasmuch as he (Dr. Angell) was engaged on behalf of Mr. Bateman as one of the chemists, and he thought that it was distinctly proved that if the powers contained in the existing Rivers Pollution Act were properly brought into exercise, a good result would follow. He could not help expressing his surprise that the author had made no mention of what was, at all events, the most recent, if not the best, development of the treatment of sewage by chemicals, namely, the method in use at Southampton. At Southampton they were using a material which entirely dispensed with lime; and upon the best authority, and also his own experience, he asserted that it was an absolute necessity if they wanted to get good results, both in the sludge and in the effluent, to do away with lime. Lime produced a very fair looking effluent at the exit from the works, but he found, both in laboratory experiments and also from information which he obtained from various places, that secondary decompositions were sure to be set up. Lime effluent, when exposed to the direct rays of the sun, would pass very rapidly through putrefactive conditions and offensive fermentations before it reached that perfect condition of purification which was indicated by the production of chlorophyl growths. He could not help smiling when he heard that so many grains of salt per gallon were added to the sewage. Salts of chlorine were already present in the sewage to such an extent that he could not see why 2 or 3 grains of salt should be added in the purifying process. Mr. Sillar had called attention to the increase of chlorine in the effluent. He could assure him that it was nothing unusual. He had seen many instances in which the chlorine had been very largely increased in passing through the land. In those cases the land had been contiguous to the sea. But the presence or absence of chlorine was not to be regarded as any indication of the noxious or innocuous quality of the effluent. The real point was the nitrogenous organic matter as indicated by the albuminoid ammonia. There was perhaps something in the remark that

the organic matter in suspension was more objectionable than that in solution. He had heard Dr. Edmunds' remarks with regard to the presence of microbes. He did not know how far Dr. Edmunds might have been experienced in watching the precipitation of microbes, but he spoke of them as if he had had something like eels to deal with. If only a few microbes were left in the effluent it would depend upon the condition of that effluent, whether the microbes would or would not speedily increase in numbers, so as to be likely to produce injury to health. Taking that fact into consideration he thought there was very little to choose between organic matter in suspension, and organic matter in solution. Reverting to the question of the use of lime, he had heard much greater men than himself condemn it. It made the disposal of the sludge a very difficult matter, for it detracted from the agricultural value of the precipitate, and led to an accumulation of the cake. An engineer in adopting a process must satisfy himself that the ultimate product of the purification was one which would not accumulate upon his hands. Perhaps it was not a difficult thing to sell the product at first, but the thing was to find whether or not the farmers would come again and again to fetch the material. In that respect he considered that they had been very successful at Southampton. He had been informed by Mr. Bennett, the engineer to the Southampton works, that at the present moment the orders which he had on hand for his mixture of sludge and street sweepings, which had not undergone pressure of any kind or any artificial treatment, either by means of exhaustion or otherwise, was very largely in excess of his supply. If, however, the sludge should accumulate on their hands at Southampton, they had put a material into it in the process of precipitation which would render it easily treated in the fires. They had proved several times that the sludge produced at Southampton by the porous carbon was very easily destroyed by cremation in the destructors, and reduced to ashes without any nuisance.

The PRESIDENT said that Dr. Angell had awakened a certain amount of curiosity with regard to the process at Southampton. He thought that the meeting would like to have some information with regard to the treatment which was carried out there.

Dr. ANGELL said he was extremely anxious not to appear to be too much of a special pleader or an advocate, but he should be very pleased to give a short description of the method. They first of all took the carbonised material and mixed it upon the works with a certain proportion of sulphuric acid. By that means the iron and the alumina which were in the

material in its carbonised state were liberated and, being soluble, were made available for the chemical treatment of sewage. At the present time this was taken in the proportion of about six grains per gallon of sewage and the sewage was treated in underground tanks. Hitherto Mr. Bennett had been using four grains to the gallon, but he (Dr. Angell), as adviser to the work, should recommend him to allow six grains. The sewage was passed into a tank of the capacity of half a million gallons, and there allowed to rest for about an hour or an hour and a half, until precipitation had taken place sufficiently to enable them to draw off the effluent water. The effluent water could be drawn off much more quickly after the addition of the chemicals than was usually the case. This was effected by means of a very ingenious arrangement made by Mr. Bennett, and which he called his floating suction. He thought that the siphon action was very dangerous, because it was sure to stir the precipitate and take some of it away. When the precipitate had gone down about two feet from the surface they began to draw off the effluent water. It would be very easy to fix a glass indicator outside to show exactly how the precipitation was going on. The effluent was passed off into the tidal waters. The sludge which was left behind was forced by compressed air into a sealed main, a mile or a little more in length, through which it passed into the sewage works proper, where the destructors were erected for the combustion and so-called destruction of all the garbage of the town. About sixty loads of this garbage or refuse from the houses were delivered every day. One of the difficulties at first was to manage on Sundays when they did not get the regular supply of refuse for fuel, but that difficulty had been overcome by a careful damping of the fires. At the present time the sludge was being mixed with the street sweepings, and it sold readily for agricultural purposes. If the sludge could not be sold there would be two ways of getting rid of it. One would be the introduction of some kind of press and the pressure of the sludge into cakes, which by virtue of the carbon and porous mineral matter present could keep sweet, and in case at any time zymotic diseases appeared in the neighbourhood and it became necessary to destroy the sludge, they could always fall back upon the destructors and dispose of it by fire.

Mr. C. Gandon asked whether he had correctly understood Mr. Astrop that from a hundred tons of precipitate or sludge it was possible to withdraw ninety per cent of moisture by the consumption of one chaldron of coke.

Mr. Astrop replied in the affirmative.

Mr. F. T. Rumble said that no doubt sludge was a very

great evil. He was at issue with the mode of treatment at Walthamstow. His method of treatment was to run the crude sewage first over the land. By that means they got rid of a great deal of the sludge, and then, if in the judgment of the managers the effluent appeared objectionable, it was diverted into tanks and treated chemically by means of the alumina cake and lime. They had had a little difficulty, inasmuch as there was a lawsuit against them for the pollution of the brook, but, by treating the effluent after the sewage had flowed over and through the earth, they had now removed most of the difficulty. So far as his experience went he preferred to have the land treatment before the chemical treatment, for it was admitted that the land acted chemically besides its action as a filter.

Mr. G. H. Ellis said it had been a source of great satisfaction to him that at Southampton the whole of the sludge which had been produced had been sold to farmers. So far as he was aware that was a result which had not been attained elsewhere. There was an arrangement for cremating the sludge in case it did not sell, but he ventured to hope that the apparatus would not be often called into operation. The other day Mr. Bennett had orders in advance for 400 tons. The price at which the sludge was sold paid a not inconsiderable proportion of the cost of treatment. The price had ranged from 2*s.* 6*d.* to 3*s.* 6*d.* for a load of sludge mixed with road sweepings. As an engineer he was somewhat interested in Mr. Astrop's method, but he thought that the point to be aimed at was a process by which sludge could be made available for farmers without the expense of drying it by artificial heat.

Mr. F. Bateman asked for particulars as to the number of inhabitants, quantity of sewage, and the cost of manual labour in connection with the works described in the paper.

Mr. Jerram, in reply, said he was certainly surprised that among both chemists and engineers, there was only one in the room who agreed with him as to the necesssity of having the chemicals exactly proportioned to the quantity and quality of the sewage to be treated. He could have no doubt that if there was a preponderance of chemicals, precipitation of the sewage which had gone into the stream previously would be caused, and this precipitation would form a black sludge at the bottom of the stream. He had experimented on that subject for several months with the water from the River Lea, and from the Thames, and with the effluent which proceeded both from Tottenham and from Leyton, and he had precipitated the organic matter in those waters. He could not but believe that such precipitation was the cause of the horrible sewage mud

which they found in all streams below sewage works. He thought that a Society like this ought to watch any proposed legislation on county or local government, with a view to leading the legislature to enact measures really effective for the control of the evils of river pollution. He hoped that when any Bill relating to sewage was brought forward, the council would communicate with the Home Secretary on the subject, with a view to obtaining a really workable measure. He had referred in the paper to the artificial filtration of sewage when there was not sufficient land available upon which to filter naturally, and he had had artificial filters in use for five years at small works. They were made with sand and gravel, and answered very well indeed. He disagreed entirely with Mr. Robinson's statement that there was no necessity for automatic action. Mr. Sillar had spoken of a human automatic machine, but he (Mr. Jerram) did not believe in such a machine. At Walthamstow they had had a man who was supposed to be awake all night mixing chemicals with the sewage and stirring up the mixture, but, upon his own admission, he used to go to sleep from about one till five in the morning, and during that period the sewage did not get treated at all. The automatic machine which he had exhibited did not require anything to keep it working but a little water and a little oil. He did not agree with Mr. Sillar as to the use of presses for drying sludge. He thought they were the most wasteful and expensive things that could be used. In the next district to his the sewage was in such a sloppy state after it left the presses that it slopped over the road. Another disadvantage was that the sewage extracted flowed from the presses charged with an excessive quantity of lime and other chemicals, and ran into the precipitation tanks, and there upset the precipitation which was going on. In the Walthamstow process they had got all the value of the liquid and all the solid too, and they did not lose anything as they did in the process of treatment by presses. Mr. Astrop's process differed from ordinary pressing in that he took the sludge pure and simple from the works, and dried it in three-quarters of an hour, thereby fixing the manurial properties, which it was impossible to do with the press. He should like to know how long Dr. Angell's process at Southampton had been at work, and what was the volume of water compared with the volume of sewage. It was very well to talk about a good effluent into the sea, but if the effluent was twice the volume of the stream into which it flowed, the effect of the effluent on the stream would soon show itself, if there was no regular treatment of it.

Dr. Angell said that the process had been at work, roughly

speaking, about nine months. As to the magnitude of the water into which the Southampton sewage flowed, there was no more difficult problem than to keep the estuarine water clean. When there was a stream always flowing in one direction, however bad the effluent which passed into it, it continually went one way; but the waters in an estuarine basin practically never changed. This was no mere theory, for he had proved it by careful observation. The tidal waters of to-day at Southampton would be the tidal waters of this day month, with the exception of the changes which resulted from evaporation and the inflow of small rivers. As to the proportion between the effluent and the waters into which the effluent flowed, that proportion was immeasurable, but before the present treatment of the sewage was adopted, the water near the outfall and at the yacht anchorage was very offensive, and numerous complaints were made about it. Since the process has been in operation a marked improvement has been observable.

Mr. JERRAM, continuing his reply, said it was known that the Aylesbury sewage was very weak, but there was no doubt that the process answered admirably. After he had made the remarks to which Mr. Sillar had referred, he went down the stream and then saw the black mud. He thought, however, that the black mud was much due to the "human automatic machine," in which he did not believe. As to the solid not being valuable there was one manure manufacturer who was making thousands of tons of manure from soil and sludge and selling it at 35*s.* a ton. He was very much surprised at Mr. Rumble's system of putting the sewage on the land before it was treated with chemicals. That seemed to him to be putting the cart before the horse, and it would not answer at Walthamstow. It would cause such a nuisance that they would not be allowed to continue it very long. They must at any rate deodorise it before they put it upon the land. He observed that Professor Kingsett had taken out a patent for treating sewage with the carbon process in powdered coke. He (Mr. Jerram) had been making some experiments on the subject, but the results were not at present ready to be laid before the meeting. One very peculiar thing with regard to sewage sludge in rivers was that at about 60° it began to give off gas bubbles; but if the organic matter in the streams themselves were taken without any admixture of sewage, decomposition did not set in without a much higher temperature.

The PRESIDENT said Mr. Jerram had given the Society an interesting paper in which he had put together in a condensed form a large amount of useful information which would prove

valuable in the 'Transactions' for reference. The author had fully described the system adopted by him at Walthamstow, and Dr. Angell had, during the discussion, given them some interesting particulars respecting the system adopted by him, in conjunction with the borough engineer, Mr. Bennett, at Southampton. He (the President) had availed himself of an opportunity, about two months previously, of going over Mr. Jerram's works at Walthamstow, and he was certainly much impressed with the excellent way in which the system of sewage disposal was arranged and carried out. More recently he had been at Southampton and had inspected the sewage works there. There also he formed a very favourable opinion of the manner in which the engineering arrangements were carried out and an equally favourable one of the results of the use of porous carbon as there employed. The whole system seemed to him to be a most perfect one, and one which commended itself for use where land could not be obtained. It might be desirable, even where land could be obtained, to make provision, not only for irrigation but also for the porous carbon process, for there were times when the land became super-saturated, or when the land did not require to be charged with sewage, as in wet seasons. That, however, was a question for those who were more practically engaged in the matter than himself, some of whom, it appeared to him, were sorely puzzled as to what was to be done for the best. The whole practice of sanitary engineering appeared to him to be beset with many difficulties. He happened to have had brought under his notice several questions relating to ventilation, respecting which very conflicting opinions had been expressed. There was also the smoke abatement question, several phases of which had been brought before the Society at different times, and to which also he had given some attention of late. Then came the sewage disposal question, which was a long way from being solved in practice as a whole. Looking at the difficulties which beset these three subjects he thought that the present mental condition of the sanitary engineer might fairly be described by an adaptation of the old schoolboy rhyme,

Ventilation is vexation,
Combustion is as bad,
The Thames and Lea, they puzzle me,
And sewage drives me mad.

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